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ARTICLE I.

Memoir on a New Metallic Acid which exists in the Red Lead of Siberia. By VAUQUELIN. *Extracted from the Bulletin of the Soc. Philom.**

BY a new examination of the red lead of Siberia, Vauquelin is convinced that this mineral contains a metallic acid very different from all those which have hitherto been known. The following are the principal results of his experiments:

The red lead ore was reduced to fine powder, and boiled in a saturated solution of carbonate of potash. An effervescence of considerable duration was produced; the powder was dissolved, but soon afterwards a precipitate fell down of a yellowish white colour. The solvent had assumed a beautiful golden colour.

The precipitate proved to be carbonate of lead.

Nitric acid was poured into the alkaline fluid till the excess of carbonate of potash was saturated. The fluid exhibited an orange-red colour. It was then mixed with a solution of tin, recently prepared, with which it assumed a brown colour, that afterwards became greenish. When poured into a nitric solution of lead, it immediately generated the red lead. By spontaneous evaporation, it afforded crystals of a beautiful orange red, besides those of the nitrate of potash.

The nitric acid being poured into the solution of the red crystals occasioned no precipitate; but when, after evaporation to dryness, the crystals of nitrate of potash, which re-

* This abstract is translated from the Journal de Physique, bearing date for Nov. 1794, but lately printed.

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mained at the bottom of the capſule, were waſhed with alkohol, a blue liquor was obtained, which after evaporation left a greeniſh blue powder, ſoluble in water, of an acid taſte, and reddening the tincture of turnſol.

The red lead ore may likewiſe be decompoſed by muriatic acid. If the latter be diluted with water, the mineralizing acid is precipitated in the form of a red powder: if it be concentrated, it re-acts on the metallic acid, deprives it of part of its oxygen, and cauſes it to paſs to a deep green colour, while vapours of oxygenated muriatic acid are diſengaged.

Theſe experiments are ſufficient to prove that the mineralizing acid of the red lead of Siberia is a new ſubſtance; but as it has ſome reſemblance with the molybdic acid, Vauquelin has made a number of comparative experiments on their alkaline compounds, which preſented very evident differences. The following are the moſt remarkable:

1. The acid of Siberian lead ore is red, when combined with potaſh; the molybdate of potaſh is white.

2. The molybdate of potaſh affords a white precipitate with the nitrate of lead, whereas the red lead ore is regenerated of a beautiful orange colour, reſembling that of the native ore when it is reduced to powder.

3. The molybdate of potaſh affords a white precipitate in flocks, when added to the nitric ſolution of mercury. The ſalt formed by the ſame alkali and the acid of Siberian red lead affords a precipitate of a deep cinnabar colour.

4. The former affords a white precipitate with the ſolution of ſilver; the latter a precipitate of the moſt beautiful carmine red, which changes to a purple red by expoſure to light.

The foregoing experiments ſufficiently prove that this new acid is metallic, and differs much from the molybdic acid. It does not differ leſs from the other newly diſcovered metals.

Uranium does not become acid, and cannot combine with the cauſtic alkalis.

Titanium is ſoluble in acids, with which it forms cryſtallizable ſalts, and does not combine with the cauſtic alkalis.

Tungſten becomes yellow in acids without diſſolving, and affords white cryſtallizable ſalts with the alkalis.

The author has not purſued this comparative examination, becauſe the properties of the other metallic ſubſtances are ſufficiently known. He promiſes to continue his reſearches as ſoon as he ſhall have procured more of this mineral.

P. S. Since this memoir was read to the Inſtitute, Vauquelin has reduced the mineralizing acid of the red lead ore. This metal is grey, very hard, brittle, and eaſily cryſtallizes in ſmall needles. The nitric acid acidifies it with conſiderable difficulty.

H. V. C. D.

Farther

II.

Farther Experiments and Observations on the Affections and Properties of Light. By HENRY BROUGHAM, *jun. Esq.**

HAVING laid before the Royal Society an account of a course of experiments † on light, in which I had been engaged, and also of the conclusions which these experiments had taught me to draw; I proceed in the following paper to relate the continuation of my observations; which, I hope, may not prove wholly uninteresting to such as honoured the former part with their attention. I am first to unfold a new and, I think, curious property of light, that may be indeed reckoned fourfold, as it holds, like the rest, equally with respect to refraction, reflexion, inflexion, and deflexion; thus preserving entire the same beautiful analogy in these four operations, which we have hitherto remarked. I shall then consider several phenomena connected either with this, or with the properties before described, and of which they afford some striking confirmations.

I.

Observation 1.—THE sun shining strongly into my darkened chamber, I placed, at a small hole in the window-shut, a prism, with its refracting angle (of 65°) upwards, so that the spectrum was cast on a chart placed at right angles to the incident rays, and four feet from the prism.

In the rays parallel to the chart, and two feet from it, I placed a pin, whose diameter was $\frac{1}{30}$ of an inch, and fixed it so that the axis of its shadow on the spectrum might be parallel to the sides of the spectrum. A set of images by reflexion was formed (similar to those described above ‡), all inclining to the violet; but what I chiefly attended to at present was their shape. I had always observed that the part formed out of the red-making rays was broadest, and that the other parts diminished in breadth regularly towards the violet. I now delineated one or two, at about three inches from the shadow; and though (from the pin's irregularities) the sides were by no means smooth, yet the general shape was in every pin, and with every prism used, nearly as represented in fig. 1. Plate VII. divided in the direction R A, according to the colours of the spectrum in which they were formed; R O B A was red, and the broadest; that is, R A was broader than O B, the confines of the red and orange; and G D E V was the violet, narrowest of all.

Observation 2.—Between the pin and the prism, $\frac{1}{10}$ of an inch from the pin, was placed a screen, through a small hole in which, of twice the pin's diameter, the rays of the spectrum passed, and were reflected into images by the pin; these were pretty distinct and well defined, when received on a chart half a foot from the pin. They were oblong, having parallel sides and confused ends; they were wholly of the colour whose rays fell on the pin, unless when the white, mixed with those at the confines of the yellow and green,

* Philosophical Transactions, 1797.

† See Philos. Journal i. 551. 585.

‡ Philosophical Transactions for 1796, page 240, or Philosophical Journal i. 557.

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caused the images to be of all the colours. When the prism was turned round on its axis, so that different rays fell on the pin, the images changed their sizes as well as their positions: they were largest when red, and least when violet.

Observation 3.—In case it may be thought that the sides of the hole, through which the rays passed in Observation 2, by inflecting, might dispose them, before incidence, into beams of different sizes, I removed the screen, and placed the pin horizontally, the axis of the shadow being now at right angles to that of the prismatic spectrum; and moving the prism on its axis, again I observed the contraction, and dilatation of the images by reflection, though now they were rather less distinct, from the greater size of the incident beam; and to shew that there was both a change of size and of place, without any manner of deception, I placed one leg of a pair of compasses in a fixed point of the spectrum, and the other in the middle point of an image formed by the violet-making rays. The prism being then moved till the image became red, I again bisected it, and found its centre considerably beyond the point of the compasses, which was indeed evidently much nearer one end of the image than the other; besides that the red image, when measured, was longer than the rest: and this satisfied me that there were two changes, one of place, with respect to the fixed point, the other of size, with respect to the centre of the image. Lastly, as far as I could judge, the dilatation and contraction appeared even and uniform.

Observation 4.—I remarked that the fringes or images, by flexion, were always increased in size when formed out of red-making rays, and were less in every other colour, and least in violet (besides being moved farther from the edge of the shadow in the former rays than in the latter); and this agrees with an observation of Sir Isaac Newton, as far as he tried it, which was with respect to deflexion. In making several experiments with prisms, I hit on a very remarkable confirmation of this. I observed on each side of the spectrum four or five distinct fringes, like the images by reflexion, coloured in the order of the spectrum, but quite well defined at the edge, and even pretty distinct at the end: they were also much narrower than those images, but like them they inclined much to the violet, and were broadest in the red, growing narrower by degrees, and narrowest of all in the violet. I moved the prism, and they disappeared; but when the prism was brought back to its former position, they also returned. I then observed the prism in open light, and saw that it had veins, chiefly opaque and white, running through it, and that there were several of these in the place where the light passed when the prism was held as before. But in case the inclination and shape of these images might be owing to the irregular order in which the veins were laid, I held another prism, which happened to have parallel veins: in many positions of this the fringes or images returned, not indeed always so regular nor always of the same kind; for some were confused and broader, formed (as I concluded from this and their position) by reflexion; others made by transparent veins and air-bubbles were also irregular, but inclined to the red, the violet being farthest from the perpendicular, and these were obviously caused by refraction; yet all agreed in this, that they were broadest in the red, and narrowest in the violet parts.

Observation 5.—I held, in the direct rays of the sun at half an inch from the small hole in the window-shut, a glass tube, free from scratches and opaque veins, but, like most glass that is not finely wrought, having its surface of a structure somewhat fibrous. When this tube

tube was ſlowly introduced into the light, and ſo held that none of the rays might be refracted, a ſtreak, chiefly white, was ſeen, ſimilar in ſhape and poſition to thoſe deſcribed before *. When narrowly inſpected, it was found to contain many images by reflexion in it. But theſe were much diluted by the abundance of white light, reflected without decompoſition in the manner above-mentioned †. This ſtreak lay wholly on one ſide of the tube; but I moved the tube onward a little, and another ſtreak darted through the ſhadow, and extended all round on both ſides: and now, when the tube was in the middle of the rays, there were two ſtreaks on both ſides, one a little ſeparated from the other and continued through the ſhadow, the other on each ſide of the ſhadow: the former was evidently produced by refraction: it contained many images very like thoſe by reflexion, only more vivid in the colours, which were all in the inverted order, the violet being outermoſt, and the reſt neareſt the point of incidence. Images ſimilar to theſe are alſo producible on the retina, as mentioned before ‡.

Obſervation 6.—I now placed a priſm at the hole, and made the ſame images by refraction, out of homogeneal light. Theſe inclined to the red, not (like images by reflexion) to the violet; but they were broadeſt in the red, and grew narrower towards the violet parts. In ſhort, when viewed beſide the images by reflexion, except in point of brightneſs and inclination, they differed from them in no reſpect.

The three firſt experiments ſhew, that when homogeneal light is reflected, ſome rays are conſtantly diſpoſed into larger images than others are, that is, into images more diſtended in length, though of the ſame breadth. The fourth experiment ſhews, that the ſame takes place when light is inflected and deflected; and the two laſt ſhew that the ſame happens when the rays are refracted in a way ſimilar or analogous to that in which the other images were produced by reflexion and flexion.

We are now to ſhew, that this difference of ſize is not owing to the different reflexibilities and flexibilities of the rays. In order to this we ſhall both demonſtrate, and then prove by experience, "that inflexion and deflexion do not decompound heterogeneous rays, whoſe direction is ſuch, that they fall on the bending body." In fig. 2. let A B be the body, GH, EF, CD, the limits of its ſpheres of deflexion, inflexion, and reflexion, reſpectively; and let I P be a white ray of direct light entering at P the ſphere of deflexion: through P draw LK at right angles to GH; IP will be ſeparated into PR red, and PV violet, and the five other colorific rays according to their deflexibilities; at R and V draw the perpendiculars ST and QO; then the alternate angles PRT, RPL; and PVQ, VPL are equal each to each. But TRP and QVP are the angles of incidence, at which the red and violet enter the ſphere of inflexion; and RPL, VPL are the angles of deflexion of the red and the violet; therefore the difference of the two latter, that is RPV, is likewiſe the difference of the two former. Suppoſe this difference equal to nothing; or that PV and PR are parallel; then rRS the angle of the red's inflexion will be leſs than vVO the angle of the violet's inflexion, by the angle RPV: (when not evaneſcent) add RPV to rRS; then rRS will be equal to vVO: that is, the divergence will be deſtroyed, and the rays enter the ſphere of reflexion, parallel and undecompounded. It is evident, therefore, that the effect ariſing from the different deflexibilities of the rays is deſtroyed by the equal

* Phil. Tranſ. 1796, page 236. or Philoſ. Journal i. 555.

† Ibid. p. 237. or Phil. Journal i. 556.

‡ Ibid. p. 243. or Phil. Journal i. 559.

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and oppoſite effect produced by their different inflexibilities; and the ſame thing may in like manner be ſhewn to happen in the return of the rays from the body after reflexion. But let the rays be ſo reflected that they ſhall paſs by the body without entering any more than one ſphere of flexion; then they will be ſeparated by their flexibilities, as we before deſcribed. It appears, then, that if the rays of light were not differently reflexible, flexion could never produce the coloured images, by ſeparating the compound light. And indeed, this may be eaſily proved by fact. At 144 feet from the bending body, the greateſt fringes by flexion are only half an inch in length, whereas the fourth or fifth images by reflexion are above half an inch at one foot from the reflecting ſurface: the one ſort is therefore more than 144 times more diſtended than the other, whereas the flexion could, at the very fartheſt, only double them. Alſo the diſtinctneſs, and brightneſs, and regularity of the colouring, are quite different in the two caſes: the ſuppoſed cauſe would neither account for the order of the colours, nor for their abſence in common ſpecular reflexion, and refraction through two priſms joined together with their angles the contrary ways. Laſtly, if we ſuppoſe the images to be produced by flexion, and then reflected from the body, it would follow that light incident on a priſm ſhould be decompounded, formed into ſeveral coloured images, and then refracted, the violet being leaſt, and the red moſt bent; all which is perfectly the reverſe of what actually happens. I have multiplied the proof of this propoſition, perhaps beyond what is neceſſary; but its great importance to the whole theory will, I hope, plead my excuſe.

Let us now ſuppoſe that a homogeneal beam paſſes through the ſpheres of flexion, it will follow that no divergence can take place from the bending power of the body; ſo that we have only to eſtimate the effect produced by the reflexion, and to enquire whether the different reflexibilities of the rays can cauſe the images to vary their ſizes according as they are formed by different rays. In fig. 3. let AB be the body, CD the limit of its ſphere of reflexion, and IP a beam of homogeneal rays, as red, incident at P and reflected to R, forming there the image Rr. It is evident that the greater reflexibility of the rays IP can only alter the poſition of the centre of Rr, making it nearer the perpendicular than the centre of an image formed by any other rays would be. But the greater length of Rr ſhews that a greater quantity of rays is reflected, or that the ſame quantity is ſpread over a greater ſpace, and that in the following way. Let IF f i be a beam of violet-making rays entering ABCD, and reflected ſo as to form the image Rv. The force exerted by AB decreaſing according to ſome law (of which we are as yet ignorant) as the diſtance increaſes, is not ſufficient to turn the rays back till they have come a certain length within ABCD. But for the ſame reaſon it turns back all that it does reflect before they come nearer than a certain diſtance: between theſe two limits, therefore, the rays are turned back. But the limits are not the ſame to all the rays; ſome begin to be turned at a greater diſtance from the body than others, and conſequently are reflected to a greater diſtance from the middle ray of the incident beam. Thus if IF f i be changed to a red-making beam, it begins to be turned back at f, and the rays fartheſt from AB are reflected to r inſtead of to v, where they fell when IF f i was violet-making; not but that the ſame quantity of rays is reflected: the only difference is, that the moſt reflexible are reflected fartheſt from the body by their greater reflexibility, and fartheſt from each other by this other property. Exactly the ſame happens in the caſe of refraction, *mutatis mutandis*; but there ſeems to be a ſlight variation in the manner in which the different rays are diſpoſed into images of different ſizes by flexion. In this caſe alſo the bend-

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ing body's action reaches farther when exerted on ſome rays than when exerted on others: but then, the direction of the rays not paſſing through the body, thoſe which are fartheſt off and at too great a diſtance to be bent, never coming nearer, are not bent at all; and conſequently as the leaſt flexible rays are in this predicament at the ſmalleſt diſtance, and the moſt flexible not till the diſtance is greater, the images formed out of the former muſt be leſs than thoſe formed out of the latter. This difference in the way in which the phenomenon appears, does not argue the ſmalleſt difference in the cauſe: it only follows from the different poſition of the rays, with reſpect to the acting body, in the two caſes. I infer then from the whole, that different ſorts of rays come within the ſpheres of flexion, reflexion, and refraction, at different diſtances, and that the actions of bodies extend fartheſt when exerted on the moſt flexible. It may perhaps be conſiſtent with accuracy and convenience to give a name to this property of light; we may therefore ſay that the rays of light differ in degree of refrangity, reflexity, and flexity, comprehending inflexity and deflexity. From theſe terms (uncouth as, like all new words, they at firſt appear) no confuſion can ariſe, if we always remember that they allude to the degree of diſtance to which the rays are ſubject to the action of bodies. I ſhall only add an illuſtration of this property, which may tend to convey a clearer idea of its nature. Suppoſe a magnet to be placed ſo that it may attract from their courſe a ſtream of iron particles, and let this ſtream paſs at ſuch a diſtance that part of it may not be affected at all: thoſe particles which are attracted may be conceived to ſtrike on a white body placed beyond the magnet, and to make a mark there of a ſize proportional to their number. Let now another equal ſtream conſiderably adulterated by carbonaceous matter, oxygen, &c. paſs by at the ſame diſtance, and in the ſame direction. Part of this will alſo be attracted, but not ſo far from its courſe, nor will an equal number be affected at all; ſo that the mark made on the white body will be nearer the direction of the ſtream, and of leſs ſize than that made by the pure iron. It matters not whether all this would actually happen, even allowing we could place the ſubjects in the ſituation deſcribed: the thing may eaſily be conceived, and affords a good enough illuſtration of what happens in the caſe of light.

Purſuant to the plan I before followed, I now tried to meaſure the different degrees of reflexity, &c. of the different rays; but though the meaſurements which I took agreed in this, that the red images were much larger than the reſt, and the green appeared by them of a middle ſize, yet they did not agree well enough (from the roughneſs of the images, and ſeveral other cauſes of error) to authorize us to conclude with any certainty "that the action of bodies on the rays is in proportion to the relative ſizes of theſe rays." This, however, will moſt probably be afterwards found to be the caſe: in the mean time there is little doubt that the ſizes are the cauſe of the fact.

II.

SEVERAL phenomena are eaſily explicable on the principles juſt now laid down.

1. If a pin, hair, thread, &c. be held in the rays of the ſun refracted through a priſm, extending through all the ſeven colours, a very ſingular deception takes place: the body appears of different ſizes, being largeſt in the red, and decreaſing gradually towards the violet. This appearance ſeemed ſo extraordinary, that ſome friends who happened to ſee it as well as

myſelf, ſuſpected the body muſt be irregular in its ſhape. On inverting it, however, the ſame thing took place; and on turning the priſm on its axis, ſo that the different rays ſucceſſively fell on the ſame parts, the viſible magnitude of the body varied with the rays that illuminated it. This appearance is readily accounted for by the different reflexity of the rays, and follows immediately from Obſervation 2d and 3d.

2. Sir Iſaac Newton found that the rings of colours made by thin plates, and by thick plates of glaſs (as he calls them), when formed of homogeneal light, varied in ſize with the rays that made them, being largeſt in the moſt flexible rays. I have had the pleaſure of obſerving ſeveral other ſorts of rings, ſo extremely ſimilar, and formed by flexion, that I can no longer doubt of this being alſo the cauſe of the phenomena obſerved by Newton. I ſhall firſt deſcribe a ſpecies, to prove "that the colours by thick and thin plates are one and the ſame phenomena, only differing in the thickneſs of the plates." Happening to look by candle light upon a round concave plate of braſs, pretty well poliſhed, ſo as to reflect light enough for ſhewing an image of the candle, I was ſurpriſed to ſee that image ſurrounded by ſeveral waves of colours, red, green, and blue, diſpoſed in pretty regular order. This was ſo uncommon in a metallic ſpeculum, that I examined the thing very minutely by a variety of experiments: theſe I ſhall not particularly now deſcribe, but give a general idea of their reſults.

It muſt be obſerved, for the ſake of clearneſs, that in the following enquiries concerning the formation of rings or fringes, the diameter of a ring or fringe means the line paſſing through the centre of that ring, and terminated at both ends by the circumference; whereas the breadth means that part of the diameter intercepted between the limits of the ring, or the diſtance between its extreme colours, red and violet.

In the firſt place, they were formed by the ſun's light in the figure of rings, ſurrounding the centre of the ſphere to which the plate was ground, at greater diſtances increaſing their breadths, the colours pretty bright, though inferior in brilliancy to thoſe of concave ſpecula.

Secondly, the order of the colours was in all red outermoſt, and violet or blue innermoſt, with a greyiſh-blue ſpot in the common centre of the whole; and on moving the plate from the perpendicular poſition, the rings moved and broke exactly like thoſe of ſpecula.

In the third place, homogeneal light made them of ſimple colours; they were broadeſt when red, narroweſt when blue and violet.

Fourthly, they decreaſed in breadth from the centre; and I found by a ſimple contrivance, that they were to one another in the very ſame ratio that the rays by ſpecula follow.

In the fifth place, I compared the general appearance of the two ſorts by viewing them at the ſame time, and was ſtruck with their general appearance, unleſs that theſe of ſpecula were moſt vivid and diſtinct.

Theſe things made me ſuſpect that they were actually cauſed by the thin coat of gums with which the ſurface of the plate was varniſhed, called lacker. Accordingly I took it off with ſpirit of wine, and found the rings diſappear; on lackering it again they returned; and in like manner I cauſed a well finiſhed concave metal ſpeculum to form the rings of which we are ſpeaking, by giving it a thin coat of lacker. This is a clear proof that theſe rings

rings were exactly the same with those of thick plates (to use Newton's expression); for the coat of gums is, when thin, pretty transparent, as may be seen by laying one on glass plates.

But this coat is extremely thin, and cannot exceed the 200th part of an inch; so that the colours of thick plates are in fact the very same with those of thin plates, except that the two kinds are made by different sized plates. We cannot, therefore, distinguish them, any more than we do the spectrum made by a prism whose angle is 90° from that made by one whose angle is 20°. This kind of colours is not the only one I have observed of nearly the same kind with those of plates; we shall presently see another much more curious and remarkable.

III.

IN reflecting on the observations and conclusions contained in my former paper, several consequences seemed to follow which appeared so new and uncommon, that I began to doubt a little the truth of the premises; but at any rate was resolved to examine more minutely how far these inferences might be consistent with fact: and I am happy in being able to announce the completeness of that consistency, even beyond my expectations. The chief consequences were the following:

1. That a speculum should produce, by flexion and reflexion, colours in its reflected light wherever it has the least scratch or imperfection on its surface.

2. That on great inclinations to the incident rays all specula, however pure and highly polished, should produce colours by flexion.

3. That they should also in the same case produce colours by reflexion.

4. That lenses, having the smallest imperfections, should produce by flexion colours in their refracted light.

5. That there should be many more than three, or even four fringes by flexion, invisible to the naked eye. And,

6. That Iceland crystal should have some peculiarities with respect to flexion and reflexion; or if not, that some information should be acquired concerning its singular properties respecting refraction.

The manner in which the first of these proportions is demonstrated a priori, is evident from the 4th figure, where CD is the reflecting surface, vo a concavity bearing a small ratio to CD, Ao and AB rays proceeding to CD. The one, AB, will be separated into Br red, and Bv violet, by deflexion from o, and will be reflected to r′ v′, forming there the fringes. The other, Ao, being reflected, will be separated into Bx and By, by deflexion from v, forming other fringes, xy, on the side of vo's shadow opposite to r′v′. Also when vo is convex instead of concave, the like fringes will be produced by the rays being deflected in passing by its sides. Lastly, when vo is a polished streak, images by reflexion will be produced, as described Phil. Trans. for 1796, p. 269. (Philos. J. i. 593.) The same passage will also shew the reason why, on great inclinations, colours by reflexion should be produced. And the second proposition, with respect to flexion, follows from what was demonstrated in this paper (p. 149 and 150); it being that case where the rays either leave or fall on the speculum at such an inclination as to come only within the sphere of inflexion, without being deflected. The

fourth propoſition is merely a ſimple caſe of flexion. And the two laſt require no illuſtration. I ſhall now relate how I inquired into the truth of theſe things a poſteriori.

Obſervation 1.—Looking at a plane glaſs mirror expoſed to the ſun's light, I obſerved that up and down its ſurface there were minute ſcratches (called hairs by workmen), and that each of theſe reflected a bright colour, ſome red, others green, and others blue. On moving the mirror to a different inclination, or my eye to a different poſition with reſpect to the mirror, I ſaw the ſpecies of the colours change; the red, for inſtance, became green, and the green blue. I applied my eye cloſe to the mirror, and received on it the light reflected from one hair. I obſerved ſeveral diſtinct images of the ſun much diſtended and regularly coloured, juſt like thoſe deſcribed above; the ſame appearances were obſervable in all ſpecula, metal and glaſs, which had theſe hairs, and I never ſaw any metal one without ſome: their ſize is exceedingly ſmall, not above $\frac{1}{1000}$ of an inch. Rubbing a minute particle of greaſe on the ſurface of the ſpeculum, images were ſeen on the fibrous ſurface; and they always lay at right angles to that direction in which the greaſe was diſpoſed by drawing the hand along it.

Obſervation 2.—Beſides theſe poliſhed hairs, many ſpecula have fewer or more ſmall ſpecks and threads, rough and black. Perhaps every poliſhed ſurface is ſtudded with a number of ſmall ones, inviſible to the naked eye from the quantity of regular light which it reflects. I took, from a reflecting teleſcope, a ſmall concave ſpeculum not very well finiſhed; its ſurface ſhewed ſeveral ſpecks to the naked eye, and many with a microſcope. Its diameter was $\frac{37}{50}$ of an inch, its focal diſtance two inches, and the ſphere to which it was ground eight inches diameter. I placed it at right angles to the rays of the ſun, coming through a ſmall hole $\frac{1}{42}$ of an inch diameter, into a very well darkened room; I then moved it vertically, ſo that the rays might be reflected to a chart 12 inches from the ſpeculum, and conſequently 10 from the focus: and though the focus appeared white and bright, yet on the chart the broad image was very different. It was mottled with a vaſt number of dark ſpots; theſe were of two ſorts chiefly, circular and oblong. Of the former a conſiderable number were diſtinct and large, the reſt ſmaller and more confuſed, but ſo numerous that they ſeemed to fill the whole image. None were quite black, but rather of a blueiſh grey, and the oblong ones had a line of faint light in the middle, juſt as is the caſe in ſhadows of ſmall bodies. But the chief thing which I remarked was the colours. Each oblong and round ſpot was bordered by a gleam of white, and ſeveral coloured fringes ſeparated by ſmall dark ſpaces. The fringes were exactly like thoſe ſurrounding the ſhadows of bodies, of the ſame ſhape with the dark ſpace, having the colours in the order, red on the outſide, blue or violet in the inſide—the innermoſt fringe was broadeſt, the others decreaſing in order from the firſt. I could ſometimes ſee four of them, and, when made at the edge of the large image, I could indiſtinctly diſcern the lineaments of a fifth: when two of the ſpots were very near one another, their rings or fringes ran into one another, croſſing.

Obſervation 3.—When the chart was removed to a greater diſtance, as ſix feet, the fringes were very diſtinct and large in proportion; alſo the ſmaller ſpots became more plain, and their rings were ſeen, though confuſedly, from mixing with one another. When the ſpeculum was turned round horizontally, ſo that its inclination to the incident rays might be greater, the diſtance of the chart remaining the ſame (by being drawn round in a circle), the

the spots and fringes evidently were distended in breadth. I have endeavoured to exhibit the sun's image, as mottled with fringes or rings and spots, in fig. 5.

Observation 4.—I placed the speculum behind a screen with a hole in it, through which were let pass the homogeneal rays of the sun, separated by refraction through a prism; this being turned on its axis, the rays which fell on the speculum were changed; the fringes were now of that colour whose rays fell, and when the rays shifted, the fringes contracted or dilated, being broadest in the most flexible rays, and consequently in those whose flexity is greatest.

[*To be continued.*]

III.

Enquiries respecting the Colouring Matter of Vegetables, and the Action of Metallic Substances and their Oxides upon them; together with a New Process for obtaining Lakes of the most intense and solid Colours. Read to the National Institute (of France) 15 Vendemiaire, in the Year VI. By the C. GUYTON.*

LINNÆUS, the great naturalist of the north, had affirmed that the red colours of vegetables announce the presence of an acid. It was long ago observed, that the juice of the violet acquires a beautiful blue shade in vessels of tin, the use of which metal was recommended in dispensatories for the preparation of violet syrup; and the original colour of such syrups as had been changed by keeping, was restored by long digestion in tin. Little attention however was paid to the cause of these phenomena; and our associate, Berthollet, in his Elements of the Art of Dyeing, had pointed it out no otherwise than by conjecture, when he supposed an acid to have combined with the oxide formed at the surface of the tin.

Such was the state of our knowledge on this subject, when, from the striking difference of colour of two preparations of the same fruit, I undertook to examine the circumstances in which these changes take place.

I suppress the detail of experiments to which I subjected almost all the acid coloured fruits in succession, such as the strawberry, the gooseberry, the plum, as well as the petals of flowers, turnsol, fernambouc, turmeric, &c. by treating them comparatively in vessels of glass, of porcelain, of metal, and metallic alloys, or by keeping them in digestion on plates of metal perfectly cleaned, or upon metallic oxides. I shall confine myself at present to such results as may improve the theory of vegetable colours, or afford some useful applications to the processes of the arts.

These experiments prove that the red colour of fruits is manifestly owing to the real action of their peculiar acid upon their colouring matter.

That tin, when it brightens or restores the colour of violets, does nothing more than resume, by superior affinity, the acid which had caused it to turn red.

That tin or its oxide is not, as has hitherto been thought, the only metal which exercises

* Translated from La Decade philos. litt. et politique, No. II. An VI. (Jan. 1798.)

 this

this affinity; but that lead, bifmuth, antimony, and zinc, produce the fame effect; that it takes place more fpeedily and completely with iron; and that the contact of all thefe metals produces a very perceptible violet tinge in fuch infufions, as without this circumftance are of a lively and decided red colour.

That the green and acid part of fruits does not contain the colouring principle which is difpofed to become red with acids; and that the coloured part retains in combination that portion of acid which is neceffary to maintain the ftate of re-action that determines the fhade.

That although this colouring principle is modified in certain vegetables fo far as to refift acid or alkaline re-agents to a certain degree, as fernambouc with regard to acids, and turnfol with regard to alkalis, yet it may be brought to this condition; which feems to fhew that it is, if not effentially yet at leaft originally, of the fame nature.

That the metallic oxides are not all equally proper to feize and fix vegetable colours; that fome among them appear to attack them with more facility, while others retain them with very little power.

Laftly, that the new metal called tungften, carried to the laft degree of oxygenation, which has not hitherto been tried in this refpect, has a decided advantage over all the other metallic oxides; that it is capable of forming lakes of great value to painters, which perfectly refift the proofs of lime-water, of acetic acid or radical vinegar, of hydro-fulphureous gas, and even to a certain point the oxygenated muriatic acid gas, that enemy of colours, which burns them fuddenly, and, according to the expreffion of Citizen Berthollet, reprefents in a few inftants the combined action of air and light. We fhall be lefs furprifed at this affertion, when it is recollected that this is the only metallic oxide which eludes the folvent power of the three mineral acids.

The oxide of tungften eafily becomes charged with the colours of all vegetable matters. I have hitherto found no more than one exception in the petals of nic-ago, of which I have not been able to extract the fine purple red, without being yet able to fufpect the caufe of this difference.

In general, the lakes formed with this oxide become deeper inftead of fainter when they are diluted. It is neceffary to foften the fhade. I have remarked that they acquire ftill more intenfity when the oxide has been previoufly rendered blue by boiling it in vinegar.

One of our affociates, Citizen Vauquelin, being informed of the object of my refearches, directed my attention to aloes. Citizen des Fontaines had the goodnefs to procure me feveral kinds I fhall fpeak only of that which bears the name of foccotrine. I made experiments on this plant, which is one of the moft rich in colour, though it does not exhibit the flighteft appearance while the equilibrium of its principles is maintained by the energy of vegetable life. The woody fibre, which is the external part, then ferves as the covering of a very vifcid matter, of a greenifh white colour, weakly acid; but fcarcely has this matter been expofed to the air before it affumes a very lively red purple colour, which becomes very abundant by the progrefs of fermentation. I have formed lakes of this matter with alumine, oxide of tin, and the white oxide of zinc. None of them were comparable to that prepared with tungften.

I do not doubt but that the oxide of this new metal may likewife be ufeful in the compofition of colours for dyeing, at leaft for the dyeing of filks, which are not intended to withftand

ſtand alkaline proofs. The oxide of tin is uſeful in theſe proceſſes, becauſe it is not eaſily attacked by acids ; but our oxide is abſolutely inſoluble.

I ſhall conclude by a reflection which may render theſe reſearches more intereſting. Wolfram, from which this oxide is obtained, is found in the territory of the (French) Republic. Several mines are already known, and the French chemiſts have been for ſome years paſt employed in ſimplifying the proceſſes by which it is ſeparated from foreign ſubſtances*.

We may therefore hope that the properties I have here deſcribed will ſupply artiſts with additional means of giving durability to the productions of genius, and may open a new branch of national induſtry.

IV.

Abſtract of a Memoir on Camphor and the Camphoric Acid, read to the Firſt Claſs of the National Inſtitute of France. By BOUILLON LA GRANGE.

[Concluded from Page 101, Vol. II.]

Habitudes of Nitric Acid with Camphor.

THE nitric acid has likewiſe a different action upon camphor from that which we have already mentioned.

The Camphoric Acid.

Koſegarten has informed us, that by diſtilling nitric acid eight times in ſucceſſion from camphor, an acid is obtained which differs in its properties from the oxalic acid.

As theſe reſults have not been confirmed by experiment, and it has not been demonſtrated that this acid is peculiar in its nature and its affinity, I have repeated the experiments of Koſegarten, of which we poſſeſs only a ſimple notice in a letter tranſlated from Crell's Journal, and printed in the 27th volume of the Journal de Phyſique, page 298. The method of preparing this acid is as follows :-

Firſt Proceſs for preparing the Camphoric Acid.

Take four ounces, or 122,284 grammes of camphor, which introduce into a glaſs retort, and pour one pound, or 489,136 grammes of nitric acid, at 36 degrees, or ſpecific gravity 1,33, and adapt a receiver well luted. Place the retort on a ſand bath, and apply a gradual heat. Much nitrous and carbonic acid gas are diſengaged ; part of the camphor riſes, while another part ſeizes the oxigen of the nitric acid. When the vapours ceaſe to riſe, unlute the veſſels ; return the ſublimed camphor into the retort, pour thereon another pound of the acid, and diſtill a ſecond time. This operation muſt be repeated until the camphor is totally acidified. Four pounds and fourteen ounces of this nitric acid are ſufficient to acidify four ounces of camphor.

When all the camphor is acidified it cryſtallizes in the remaining fluid. The whole muſt then be poured on a filter after previous decantation of the acid, and diſtilled water

* Journal des Mines, No. XIX. G. Our Corniſh mines alſo afford the ores of tungſten. N.

muſt

muſt be thrown on the cryſtals in order to clear them of the portion of nitric acid which may adhere to their ſurface. The moſt certain indication that the camphor is acidified, conſiſts in its cryſtallization by the cooling of the fluid which remains in the retort.

Purification of the Camphoric Acid.

This acid is purified by ſolution in hot diſtilled water, filtration and evaporation of nearly half the fluid, or till the period at which a ſlight pellicle is formed. The cryſtals of camphoric acid ſeparate by cooling.

A Second Proceſs.

Another method of procuring the camphoric acid conſiſts in the uſe of nitric acid at 50 degrees, or ſpecific gravity 1,532. This proceſs is much ſpeedier than the former, but its inconveniences are ſuch as do not entitle it to be preferred to the other. In fact, the camphor is more ſpeedily attacked by the concentrated acid; but inſtead of ſubliming, it paſſes over into the receiver, and a portion is likewiſe carried off in the gas which eſcapes. Theſe circumſtances occaſion a real loſs in the product, as may be ſeen by the following tables of the reſults:

By the firſt method.	grammes.
Nitric acid, at 36 degrees, 4 lb. 14 oz. or - -	2114,538
Camphor, 4 oz. or - - - - - -	122,284
Acid obtained, 14 gros, or - - - -	53,498
By the ſecond method.	
Nitric acid, at 50 degrees, 2 lb. 12 oz. 2 gros, or - -	1352,168
Camphor, 4 oz. or - - - - -	122,284
Acid obtained, 9 gros, or - - - -	34,392

SECT. VII.

Camphor and Oxygenous Gas.

BEING deſirous of knowing how camphor would be affected with oxygenous gas, I made the following experiment:

Oxygen gas was obtained from the ſuperoxygenated muriate of pot-aſh. After an inverted glaſs veſſel was thus filled, it was transferred to the trough containing mercury, and a ſmall portion of water was paſſed to the ſurface of the metallic fluid.

On the other hand, a ſmall piece of camphor with a particle of phoſphorus was placed in a ſmall cupel. A tube was then bended in ſuch a manner and applied, that one of its extremities was placed beneath the glaſs jar, and the other in a pneumato-chemical trough beneath a jar filled with water.

In this diſpoſition of the apparatus the phoſphorus was ſet on fire by means of a red-hot iron. The inflammation was communicated to the camphor. Much caloric was diſengaged with a very brilliant flame; the inner ſurface of the veſſel became covered with a black matter, which was detached by degrees, and floated upon the water over the mercury. A gas was collected at the ſame time, which exhibited all the characters of carbonic acid gas.

This experiment therefore confirms the reſult in Section II. for it cannot be doubted but that the matter in the retort was a true carbon.

The

The water which had been placed under the jar became very odorant, and its ſmell was abſolutely the ſame as that of the oil obtained by the proceſſes already deſcribed. It was acid, and reddened the tincture of turnſol. Lime water aſcertained the preſence of carbonic acid in this water; but upon adding an exceſs of the carbonic acid the precipitate was not taken up; which fact led to a ſuſpicion that another acid was preſent, and in fact a calcareous camphorate was obtained.

I obſerved that it was neceſſary to add a ſmall portion of phoſphorus to the camphor, which otherwiſe would not have taken fire. For an ignited body only diſſipates it in vapour, and it cannot be inflamed but by the contact of a body at a much more luminous or higher degree of heat.

SECT. VIII.

Characters of the Camphoric Acid.

THE camphoric acid has a ſlightly acid bitter taſte, and reddens the tincture of turnſol. It is cryſtallizable, and a maſs of its cryſtals reſembles the muriate of ammoniac. By expoſure to the air it effloreſces. Cold water diſſolves it with difficulty. One ounce of water at the temperature of 10 or 12 degrees of Reaumur, takes up no more than ſix grains; whereas at the boiling heat it diſſolves 48 grains. When this acid is placed on ignited coals it emits a denſe aromatic fume, and is entirely diſſipated. By a gentler heat it melts and is ſublimed.

If the camphoric acid be put into a porcelain tube (heated), and oxygen gas be paſſed through, the acid does not undergo any change, but is ſublimed.

By mere diſtillation our acid firſt flows and then ſublimes; by which proceſs its properties are in ſome reſpect changed. It no longer reddens the tincture of turnſol, but acquires a briſk aromatic ſmell; its taſte becomes leſs penetrating, and it is no longer ſoluble either in water or the ſulphuric and muriatic acids. Heated nitric acid turns it yellow and diſſolves it. Alcohol likewiſe diſſolves it; and if this ſolution be left in contact with the air of the atmoſphere it cryſtallizes.

Camphoric acid does not produce any change in ſulphur; alcohol and the mineral acids totally diſſolve it; and ſo likewiſe do the volatile and the fat oils. It forms combinations with earths, alkalis, and metallic ſubſtances. We ſhall give a more particular account of theſe ſaline combinations in another Memoir.

Among metallic ſolutions, it decompoſes only the ſulphate and the muriate of iron.

It produces no change in the ſolution of indigo by ſulphuric acid, nor in the tincture of nutgalls. It has no action on lime-water.

Concluſion.—From all theſe facts it follows, that camphor is a volatile oil rendered concrete by carbon; that theſe two diſtinct products may be obtained by the medium of alumine; and laſtly, that by treating camphor with the nitric acid, another peculiar acid is obtained which differs from all the known vegetable acids,

1. By its cryſtallization. 2. Its ſparing ſolubility in cold water. 3. Its burning without leaving any reſidue. 4. Its not precipitating lime-water. 5. Its producing no change in the ſulphuric ſolution of indigo—a difference which remarkably diſtinguiſhes it from the ſuberic acid which turns the ſame ſolution green; and 6. By the formation of peculiar ſalts, every one of which exhibit a blue flame with the blow-pipe.

V. An

V.

An Account of several new Experiments on Heat, with occasional Remarks and Observations; and Conjectures respecting Chemical Affinity and Solution, and the Mechanical Principle of Animal Life. By BENJAMIN, *Count of* RUMFORD *.

AT the end of a French Translation of the first edition of Count Rumford's Seventh Essay, by Professor Pictet, that Translator added the following extract of one of the Count's private letters to him, dated June 9, 1797: "I should have been much surprised if my "Seventh Essay had not interested you; for in my life I never felt pleasure equal to that I "enjoyed in making the experiments of which I have given an account in that perform- "ance. You will perhaps be surprised when I tell you, that I have suppressed a whole chap- "ter of interesting speculation, merely with a view of leaving to others a tempting field of "curious investigation *untouched*, and to give more effect to my concluding reflection, "which I consider as being by far the most important of any I have ever published." As these assertions were not originally intended for the public eye, the worthy author found his situation altered with regard to the philosophical world by this communication; and with a very honourable degree of delicacy conceived that it became his duty to let the public know with precision how far he had carried his enquiries in the investigation of the subject of that Essay, instead of giving obscure hints of important facts kept in reserve, and apparently to be brought forward, when others might make discoveries of the same kind. The work before us contains the matter in question.

The experiments and observations contained in this Second Part are as follow:

CHAP. I. When a quantity of water was frozen in a glass jar by placing the vessel in a freezing mixture, it was always observed that, as the ice first began to be formed at the sides of the jar, and gradually increased in thickness, the portion of water in the axis of the jar which last retained its fluidity, being compressed by the expansion of the ice, was forced upwards towards the end of the process, and formed a pointed projection or nipple, which was sometimes above half an inch higher than the rest of the upper surface of the ice. This fact induced the Count to make experiments relative to the defect of conducting power in fluids downwards. For, if a fluid be poured upon this cake of ice so as to cover the whole mass, and a heated solid be then suspended at a small distance from the papillary protuberance, it is evident that this last, if melted, will derive its fluidity from the transition of heat, through the fluid of which the conducting power or its absence was meant to be ascertained.

In an experiment with fine olive oil, the cake of ice or congealed water was three inches thick, four inches and three quarters in diameter, and the pointed projection rose half an inch above the upper surface. The temperature of the apartment was 31 degrees of Fahrenheit, and the external part of the jar as high as the ice was surrounded with a mixture of pounded ice and water. In this situation of the apparatus, fine olive oil, previously cooled to 32 degrees, was poured into the jar till it stood at the height of three inches above the surface of the cake of ice.

* Abridged from the Second Part of his Seventh Experimental Essay.

A solid

A solid cylinder of wrought iron 1¼ inch in diameter, and 12 inches long, provided with a hollow cylindrical sheath of thick paper, was heated to the temperature of 210 degrees in boiling water, and, being suddenly introduced into its sheath, was suspended from the ceiling of the room, and very gradually let down into the oil, until the middle of the flat surface of the hot iron, which was directly above the point of the conical projection of ice, was distant from it only 2-10ths of an inch. The end of the sheath descended 1-10th of an inch lower than the end of the hot metallic cylinder. The ice was perfectly visible through the oil in every stage of this experiment. How long the metallic cylinder was kept immersed in the oil is not said, but the time was no doubt sufficient to satisfy the Count that heat is not propagated downwards through this fluid. The ice was not in the smallest degree diminished, or otherwise affected, by the vicinity of the hot iron.

A similar experiment was made with a fresh cake of ice in the same jar, but with ice-cold mercury covering the cake to the height of about an inch. The surface of the mercury in the jar was cleaned with blotting paper, after which the whole was suffered to remain quiet for about an hour, when the hot cylinder of iron was very carefully introduced, and suffered to remain several minutes at the distance of a quarter of an inch from the point of the conical projection of ice.

In this as well as in the other experiment, the cylindrical sheath was made to project 1-10th of an inch below the base of the iron, for the purpose of diminishing the internal motions of the fluid.

In this experiment also the ice remained unchanged. In order to shew whether the ice were really in the state of melting with the least possible addition of heat, the Count touched it with his finger beneath the mercury; and he found that this operation could not be so speedily performed, but that signs of water having been produced became apparent on the clean and bright surface of the mercury.

From the results of these experimental investigations, it appears to our author that water, oil, and mercury, are perfect non-conductors of heat; and that when either of these substances takes the form of a fluid, all interchange and communication of heat among its particles, or from one of them to the other directly, become from that moment *absolutely impossible* *. In the Philosophical Transactions for 1792, the Count has shewn the extreme imperfection of the conducting power of air, and in his Sixth Essay he has shewn how much reason there is to conclude that the particles of steam and of flame are in the same predicament: From all which circumstances he is disposed to conclude, that it is common to all fluids, and even essential to fluidity, that they shall not immediately or directly conduct heat from particle to particle.

This important circumstance was applied by our author, in the Philosophical Transactions above referred to, to account for the warmth of natural and artificial clothing, and of snow, as well as to explain various phenomena of winds. And in his Sixth Essay he availed himself of the non-conducting power of steam and of flame, to explain the effects of the blow-pipe, and investigate the most advantageous forms for boilers. And, lastly,

* On the absolute impossibility of the transition of heat through fluids at rest, see the note in our Journal, I. 291. It may however be observed, that most of the Count's conclusions will be practically true; that is to say, with regard to such terms of time as his experiments justify. N.

in the third chapter of the present Essay, he has extended the application of his discoveries to the means which appear to have been used by the Creator of the world, to render the temperatures of different climates more nearly equal by means of the salt water, which covers so large a part of the surface of the globe. But the most interesting application remains to be made of this doctrine to chemistry, vegetation, and the animal economy; to the learned in which branches of science he begs leave most earnestly to recommend them. From his meditations he thinks it not improbable that every change of state, in every kind of substance, may be owing to heat alone; that every concretion is a true congelation effected by cold, or the diminution of heat; that every change from a solid to a fluid form is a real fusion;—and that it may be found, that the apparent violence with which certain solids are attacked by their solvents, is not owing to any particular or elective attraction, but to the considerable degree of heat or cold, and the great difference of specific gravity which ensues in the solvent, from this cause as well as from the subsequent change produced by combination.

If fluids be non-conductors of heat, it will necessarily follow, that change of temperature will produce currents in every chemical solvent, in proportion to the change of specific gravity; and the rapidity of the process of solution will be proportioned to that of the currents, or to the change of temperature. And again, if the saturated solution be either heavier or lighter than the solvent itself, currents will be produced from this cause also, which will tend to render the solution more or less rapid, according to the magnitude of this difference, and its direction with regard to the other difference caused by the change of temperature. An instance of these two causes operating jointly is adduced in the solution of common salt in water. If this solid be supported in a perforated vessel under water, but near its surface, the solution will be most rapid: first, because the temperature is diminished, and consequently the water is condensed in the process; and, secondly, because the solution of salt in water is itself more dense than the water itself. On both accounts therefore the brine will rapidly descend, and fresh portions of the solvent will continually be brought in contact with the salt.

On this occasion the author proposes a curious question: Whether, in a case where the expansion by heat were equal to the condensation by the process of chemical union, the effect of solution could take place?—He answers by observing, that if chemical attraction, as has been generally supposed, really exists and operates in the way of predilection beyond the point of actual contact, it is probable that the solution would take place.—But if this attraction be nothing more than has been just pointed out, it would follow that though solution would not be absolutely impossible, yet it would be so slow as hardly to be perceptible.

In the consideration of the solution of salt in water, the position of the solid near the surface of the fluid was stated as a circumstance essential to its rapidity. The uniform diffusion of solids through the whole mass of lighter fluids which dissolve them, has usually been considered by chemists as a proof of attraction between the two bodies operating at a distance from the place of contact. It seems to be a striking result of the Count's reasoning, that if no other causes should interfere but those which flow from the mutual combination of the two substances, the salt would not, at least in any moderate portion of time, be carried to the upper part of the fluid resting upon it. He made this the subject of a particular experiment.

A cylin-

A cylindrical glaſs jar 4½ inches in diameter, and 7¾ inches high, was placed in the middle of another cylindrical glaſs jar 7½ inches in diameter, and eight inches high, which ſtood in a ſhallow earthen diſh, nearly filled with pounded ice and water. This apparatus was placed in an uninhabited room, where the temperature was conſtantly at about 36 degrees of Fahrenheit. In this ſituation, a quantity of pure ice-cold water, ſlightly tinged with turnſol, was poured into the inner jar, to the height of more than two inches; and then by means of a glaſs funnel, which ended in a long narrow tube, by introducing this tube into the freſh water, and reſting it on the bottom of the jar, a quantity of the ſtrongeſt clear ice-cold brine of common ſalt, equal to that of the freſh water, was poured very ſlowly in. The difference of colour rendered the two fluids very diſtinguiſhable, and ſhewed that they were not diſpoſed to mix together.

The ſpace between the two jars was then filled with large fragments of ice and ice-cold water, becauſe pounded ice would have obſtructed the view; and when this was done, the tinged water of the interior jar was carefully covered with ice-cold olive oil to the height of about an inch. This laſt fluid ſerved to prevent the water from being agitated by the air, or cooled by evaporation or the communication of heat.

The fluids remained in perfect tranquillity, without the ſmalleſt diſpoſition to mix together, during four days. At the end of that time the ſmalleſt jar was removed without agitation, and placed in the window of a room heated by a German ſtove. In leſs than an hour it was perceived that the brine and the tinged freſh water began to mix, and at the end of 24 hours they were intimately mixed throughout.

The author leaves philoſophers to deduce their own concluſions from this experiment; but in the mean time points out a reſult, which is not only curious in itſelf but capable of affording important conſequences. He thinks there are ſtrong reaſons to conclude, that were a lake but very deep, its waters near the ſurface would neceſſarily be freſh, even though its bottom ſhould be one ſolid maſs of rock-ſalt. He ſuggeſts the advantage which might accrue to an inland country where ſalt is ſcarce, if on experiment it ſhould be found that the water at the bottom of ſome deep lakes is ſalt. And that the water at the bottom of all very deep lakes ought neceſſarily to be ſalt, even in ſituations where no mines of ſalt exiſt, appears to him probable, from the geological facts which indicate that moſt of our continents have been covered by the waters of the ocean. If ever that event happened, he thinks it highly probable, that the ſalt water left at the bottoms of all deep lakes by the ſea, on its retiring, muſt be there now.

Chap. II. In the former part of this Eſſay, the author has availed himſelf of all the circumſtances which accompany the cooling and congelation of water, to account for the ſtriking effects which they produce in the economy of heat on the ſurface of the globe. As water contracts by cooling to the 41ſt degree, and afterwards expands until it aſſumes the ſolid ſtate; it ſhould ſeem at firſt conſideration, that, when the heat is abſtracted from the bottom of a veſſel containing this fluid, the particles of water would riſe and elude the refrigerating power until the whole maſs was cooled to that temperature, and afterwards continue to circulate in the ſame manner as if the temperature were actually riſing, until at length the whole maſs having arrived at the temperature of 32°, it would become ſolid almoſt inſtantaneouſly. But theſe inferences ſuppoſe the particles to circulate with leſs impediment from inertia and reſiſtance than would be ſufficient to detain them long enough at the lower

ſurface

ſurface to effect their congelation. Whether this ſuppoſition may agree with the actual ſtate of things or not, might perhaps be aſcertained by reaſoning from former facts; but in all ſuch queſtions where the deciſion can be immediately and ſimply had from experiment, it is certainly beſt to apply to that ſource of information. Count Rumford made the trial, by pouring mercury into a glaſs tumbler to the depth of one inch, and upon this about the ſame bulk of water. Both fluids were at the temperature of 60 degrees. The tumbler was then placed in a freezing mixture of ſnow and common ſalt, which reached no higher than the upper ſurface of the mercury. The ice was formed at the bottom in contact with the mercury, not only under theſe circumſtances, but alſo when the experiment was varied by previouſly cooling the mercury to about ten degrees, and then gently pouring boiling hot water on its ſurface. This laſt fluid was inſtantly frozen, and gradually formed a thick cake of ice, covering the mercury, though almoſt the whole of the maſs of the unfrozen water which reſted on this ice remained nearly boiling hot.

Among the inferences deduced from this experiment, Count Rumford makes one, to account for the formation of ice at the bottom of rivers, which he thinks can only take place in ſuch ſtreams as do not conſtantly fill their bed, but occaſionally overflow portions of ground cooled by the atmoſphere below the freezing point*. Another important inference from the ſame facts is, that it is impoſſible any fluid ſhould be of the ſame temperature while expoſed to light, though its maſs be ever ſo ſmall, and that at the difference of heat muſt occaſion perpetual motions among its parts. This conſequence is very fully explained in detail by our author, who conſiders fluidity as the life of inanimate bodies, and congelation as the ſleep of death; and is thence diſpoſed to reject altogether the attribution of attractive powers or exertions of any kind to dead motionleſs matter. He extends his meditations to the vital principle in living animals, and demands whether their life alſo do not depend on the internal motions in their fluids occaſioned by an unequal diſtribution of heat? and whether ſtimulation be not in all caſes the mere mechanical effect of the communication of heat?—The ancient hypotheſis, that the life of an animal reſides in its blood; the evident tendency of reſpiration, digeſtion, and inſenſible perſpiration to produce and perpetuate inequalities of tem-

* I am not acquainted with the peculiar circumſtances under which ground ice is formed; but it is certainly poſſible that ſuch an event ſhould happen in a ſtream which conſtantly fills its banks. Suppoſe a ſtream to flow with very little agitation in contact with an atmoſphere eight degrees or more beneath the freezing point. It is known that water whoſe parts are relatively at reſt, or nearly ſo, may be cooled about eight degrees below 32° without aſſuming the ſolid ſtate. The middle of our ſtream might therefore continue fluid when ſo cooled, and the congelation would only take place at the ſides, where the friction againſt the banks would cauſe the requiſite internal agitation. The middle cold ſtream, on account of its expanſion beneath 41 degrees, would occupy the ſurface, and conſequently could not rub againſt the bottom except in ſhallow places, or unleſs ſome means were to offer of ſinking it. Suppoſe one or more ſprings riſing from a great depth in the earth, and thence poſſeſſing the mean temperature of the climate, to flow into the principal ſtream. This maſs of warm water would occupy the ſuperior part of the ſtream, and cauſe the cold maſs to deſcend; and wherever this touched the bottom it would be agitated, and form a coat of ice. Theſe effects would be governed by the temperatures, the maſſes, and the local requiſites, for which upon the whole we are in want of obſervations. As to the aſſumed facts, they are undoubtedly poſſible. A remarkable inſtance of a warm ſtream in winter is ſeen in the ſmall river Wandle, which burſts out of the earth near Carſhalton in Surrey, and after flowing with conſiderable velocity over a line of ten miles, and giving activity to thirteen mills, falls into the river Thames at Wandſworth without having had time to acquire the freezing temperature, even in the ſevereſt weather. N.

perature;

perature; the effects of heat and cold introduced into the ſyſtem in liquid foods, with various other phyſiological events, are adduced by our author in ſupport of the probability of that doctrine which he recommends to the attention of philoſophers.

Chap. III. In the third chapter, the Count proceeds to point out a variety of ſtriking conſequences and obſervations which flow from the imperfect conducting power of fluids. One of the moſt immediate of theſe is the very great degree of heat which may exiſt at a ſmall diſtance from other bodies intenſely cold. Thus it is found that ice evaporates while expoſed to an atmoſphere in which the thermometer ſtands far below 32 degrees; an event which he rationally accounts for, by ſuppoſing that ſome of the particles of air which come into contact with the ice are ſo hot as not only to melt thoſe particles of ice which they happen to touch, but alſo to reduce part of the generated water to ſteam before it has time to freeze again; or otherwiſe by ſuppoſing the ſame effect to be produced by the intenſe heat generated from the abſorption of light by ſmall projecting points of the ice. He even thinks the metals would evaporate if they were bad conductors of heat, inſtead of being very good conductors as in fact they are; and in proof of this he urges the fact, that mercury, which from its fluidity is what he would call a non-conductor, is known to evaporate by the mere heat of the atmoſphere.

That the moſt intenſe heat is often excited in the midſt of maſſes of cold liquids, is not to be doubted. The ſun's rays generate heat of extreme intenſity; but when circumſtances are not favourable to its accumulation it is ſoon diſperſed, and leaves no traces of its exiſtence which can be meaſured either by inſtruments or the organs of ſenſe. The Count does not think it improper to infer, that the heat excited by a ray of light in an indefinitely ſmall particle of ſolid and opaque matter floating in a maſs of cold water, may be equally intenſe with that which is generated in the focus of the moſt powerful burning mirror or lens. Hence he accounts for various effects of the ſun's light, which gradually produce changes of the ſame nature as thoſe which ariſe from very elevated temperatures. Thus wood is rendered brown or ſuperficially charred, luna cornea is rendered black, or as may be ſuppoſed ſuperficially reduced, metallic oxyds are deprived of oxygen, the green leaves of vegetables emit the ſame fluid, &c.

Among other familiar inſtances of intenſe heat, in circumſtances where no viſible ſigns appear, the Count adduces that of the aſcending current of air from a candle. Iron is fully red hot at the temperature of about 1000° of Fahrenheit's ſcale; braſs melts at 3807°, copper at 4587°, ſilver at 4717°, and gold at 5237°; and it muſt be obvious that this laſt temperature obtains where gold undergoes fuſion. But fine gold, ſilver, or copper wire flatted, ſuch as is uſed to cover thread to make lace, melts inſtantly on being held in the flame of a candle, or even if held for a few ſeconds over the flame at the diſtance of an inch, where there is no appearance of fire or ignition. The air, or many of its particles, muſt therefore be heated to this intenſity, though their number may be inſufficient to cauſe any very elevated degree of temperature in a large maſs of metal or a thermometer.

From this hypotheſis of intenſe heat in the ſmall parts of fluids, or bodies ſuſpended in them, it will follow that chemical ſolutions and precipitations are or may be effected by the ſole agency of heat, and will not differ from fuſion and congelation;—that the points of temperature at which bodies aſſume the ſolid, the fluid and the gaſeous ſtates will be of the utmoſt conſequence with regard to theſe events, and that perhaps there may be no other eſſential

difference

difference between one body and another but what arises from this circumstance;—that these points being changeable by combination, give rise to a prodigious number of consequences different from those which obtain in the simpler elements. We must refer to the original Essay for the more ample elucidation of these points, where chemical philosophers will see the outline traced of an immense field of research, which, if it should not include the whole of the phenomena which have been designated under the name of elective attraction, will certainly comprehend a large mass of effects which never fail to present themselves in every natural change which comes under our notice.

Chap. IV. The last and concluding chapter contains a variety of miscellaneous experiments. 1. If a thermometer with a long cylindrical bulb being at the temperature of the air in summer, or any temperature above the freezing point, be plunged to half the length of its bulb in ice and water, the mercury will fall in the tube only half as much as if the whole bulb had been immersed. This experiment succeeds equally well, when the superior half of the bulb is covered with a sheath lined with soft fur to prevent the communication of heat from the air during the experiment. It shews that the upper half of the mercury is not cooled, or, in other words, that heat does not pass downwards in fluids.—2. Ice-cold water standing on ice at the bottom of a thin tube of glass, may be boiled by holding the tube inclined over the flame of a candle applied near the upper end, and gradually removed lower down as the ebullition proceeds. In this way all the water in the tube may be brought into the most violent ebullition, to within a quarter of an inch of the ice, before this begins to be melted.—3. The radiant heat from a red hot iron bullet was not found to make its way downwards through liquid water, nor through melted tallow, nor melted wax. The experiment was tried on a thermometer about a quarter of an inch beneath the surface of each of these fluids. These facts are analogous to the observation of Scheele, who found that radiant heat does not pass through glass. When the red-hot ball was held over a cake of ice, the heat appeared to be transmitted by communication, though very slowly. The excavation in ice was deepest near the middle. The contrary was the case both in tallow and in wax. Count Rumford does not directly explain the cause of this remarkable difference. It is the only immediate fact I recollect having seen in his Essays, in proof of the assertion that water stands alone, with regard to the property of expanding in the lower degrees of refrigeration; and consequently of contracting as its temperature increases in that part of the scale. From this property it would follow, that a warm stream of water must descend immediately beneath the bullet, while an ascending current obtains near the circumference of the cavity. But if the expansion of tallow be uniform from the earliest point of fusion, the heated fluid will not descend, but will spread out sideways, and circulate in a contrary direction to the current in water; while the ascending stream of cold fluid will be near the centre, and will defend part of the tallow in that region from being fused.—4. Beautiful crystals of sea salt were formed in brine standing on mercury in an open vessel for half a year. The Count makes it a question, whether the mercury contributed in any manner to this effect? It seems probable that it did not, and that evaporation equally slow and undisturbed would have afforded the same result in other circumstances.—5. A similar remark may be made on his observation of olive oil rendered colourless by exposure to the air for six months upon brine, in a place where the sun's rays never entered. The same effect happens to olive oil which remains in unclosed vessels in work-shops.—6. Part of the last-mentioned

tioned colourless oil being imperfectly congealed, an attempt was made to cause radiant heat to descend through its mass. It was expected that the transparent or melted portion of the oil would in this case have been hemispherical, or at least of some convex figure; but this did not happen, for the surface beneath the fluid or melted part continued flat. Whence the Count infers, that the fusion was effected merely by the heat absorbed or generated by the sides of the tumbler.

7. The concluding experiment of this Essay affords a striking result of the circulation of fluids under different circumstances. When the instrument described in our Journal, I. 342, was placed by accident in the window of a room warmed by a German stove, the difference of temperature, on the side nearest the window from that opposite the internal part of the room, was such as to keep up a constant circulation of the fluid. This event induced the Count to substitute a box of plate glass in the place of one of the panes of his window. This apparatus, which was 13 inches high, 10½ inches wide, and one inch within from plate to plate, was half filled (and as I suppose afterwards completely filled) with the saline solution with pieces of amber floating in it, as described at the place last quoted. The Count expected to behold the currents as usual in this new vessel; but to his great surprise they proved not vertical, but horizontal, exhibiting actual winds in opposite directions, which springing up in the different regions of this artificial atmosphere, prevailed for a long time with the utmost regularity, while the small particles of the amber collecting themselves together formed clouds of the most fantastic forms, which being carried by the winds rendered the scene perfectly fascinating.

Several subordinate circumstances gave these motions a more intimate resemblance to the atmospherical phenomena. One remarkable appearance never failed to present itself regularly every day during the three weeks that the experiment was continued. The clouds, after having been driven about all day by the different currents, in the liquid (of which there were sometimes as many as six or seven running in opposite directions at the same time), never failed to collect themselves together in the evening into large masses, sometimes forming only one, and sometimes two or three strata at different heights, where they remained to all appearance perfectly motionless during the night.

The experiment was put an end to by the accidental breaking of the vessel.

VI.

An Account of certain Motions which small lighted Wicks acquire when swimming in a Bason of Oil; together with Observations upon the Phenomena tending to explain the Principles upon which such Motions depend. By PATRICK WILSON, *F.R.S. Edin., and Professor of practical Astronomy in the University of Glasgow* *.

Glasgow College, April 28, 1795.

DEAR SIR,

I NOW sit down to give you some account of the little hydrostatical lamp which I so briefly mentioned to you in a former letter. As I am far from being sure whether what I have

* In a Letter to Mr. John Playfair, F. R. S. Edin. &c. inserted in the Edinburgh Transactions, vol. iv.

have to offer upon this subject may be entitled to the notice of the Edinburgh Royal Society, so I will refer this point to your determination after you have had leisure to consider the contents.

The phenomena treated of in the sequel were quite new to me a few months ago, and, so far as I know, have not hitherto been attended to or described by any body else. What I have called the hydrostatical lamp, consists of a small circular patch of common writing paper, about three-eighths of an inch in diameter, having about a quarter of an inch of soft cotton thread standing up through a puncture in the middle to serve as a wick; and the phenomena in question are certain motions which such minikin lamps acquire when lighted and made to swim in very pure sallad oil.

A shallow glass bason, with sides rising nearly perpendicular, or a common glass salver, will conveniently contain the oil for these experiments. As soon as the lamp is lighted, it will immediately sail briskly forward in some direction till it meets the side of the vessel, and afterwards will take a circular course, always bearing up to the sides, and so will perform many revolutions.

Sometimes the circulation is from right to left, and sometimes in the contrary direction, according as that point of the paper base, which in the direct sailing kept always foremost, turns away from the side of the glass, a little to the right or to the left hand of that which comes to be the point of contact. This turning away of what may be called the leading point of the base is distinctly observable by a partial rotation of the lamp round the wick as an axis, as soon as it arrives at the side of the vessel. Sometimes, though rarely, the leading point itself attaches itself to the side, and forms the vinculum, in consequence of the well-known corpuscular attraction between the elevation of oil around the base and that belonging to the sides of the glass; and when the vinculum so corresponds to the leading point, the lamp will be found to stand still without any tendency to circulate.

When the little wick has any sensible eccentricity upon the circular paper base, the lamp will sail so as to make that part of the base which lies nearest to the wick the stern; and if the base of the lamp be clipped to an oval form, and the wick placed in the longer axis excentrical, that end of the base nearest the wick will also keep hindermost, when the lamp sails across the salver. In the same manner, if there be an equilateral triangle, having its wick in the perpendicular which bisects any of the sides, either the vertex or side will become the stern, and keep hindermost, according as the wick is placed nearest the one or the other. Lamps so constructed are found also to circulate upon their arrival at the side of the vessel, when the leading point turns away from the glass, as it commonly happens.

Whatever be the cause of the sailing of the lamp directly forward, the perpetual circulation after it arrives at the side seems to proceed from the force which formerly impelled it still acting in the same manner, but in a direction inclined to that of the corpuscular attraction which forms the vinculum; and it is evident that this inclination will be greater or less according as the leading point is more or less averted from the glass. When it so happens that the leading point and vinculum coincide, it should seem that both forces just now mentioned must urge the lamp in a direction perpendicular to the side of the glass; in which case it must stand still, agreeable to observation.

The next thing which I had occasion to take notice of when the lamp sailed in a direct course, was a seemingly very active repulsion between its stern and the oil at the surface contiguous

tiguous to it. This became manifeſt when very fine charcoal duſt was lightly ſcattered around the lamp. As it then proceeded in its courſe, it marked out a ſpreading or diverging *wake* behind it entirely clear of all duſt, in conſequence of the particles being chaſed backwards and laterally with a motion much more than merely relative.

Deſirous of learning how this diſpoſition of the duſt would take place when the lamp was ſtationary, I conſtructed one of a fine wafer, and with an excentric wick conſiſting of a ſoft cotton thread doubled; and to prevent the wafer or baſe from catching fire I coated its upper ſurface with gold leaf. When this was made to reſt immoveably upon the oil, the duſt retired in all directions ſo as to leave the ſpace adjacent to the wafer quite free from every particle. But here it was obſervable, that this diſperſion of the duſt, by the ſeeming repulſion of the baſe of the lamp, was much more rapid at that ſide which lay neareſt to the wick than at any other part, and leaſt of all ſenſible at the ſide diametrically oppoſite.

The circumſtances laſt mentioned ſeem ſufficiently to account both for the progreſſive motion of the lamp, and for the general law of this motion formerly deſcribed. For, regarding this diſperſion of the duſt as yet only in a general way, and as the effect of ſome repulſion between the baſe and the oil contiguous to it, the facts above mentioned plainly indicate, that in all caſes this repulſion is ſtrongeſt at that part of the baſe neareſt the wick or flame; and as action and reaction are equal and contrary, the lamp muſt therefore be impelled, in the direction of a line drawn through the wick, towards that part of the baſe moſt remote from it, and where the reaction is the leaſt.

But in order to obtain a ſtill more competent knowledge of the phyſical cauſe of theſe motions, it ſeemed now neceſſary to enquire more particularly into this apparent repulſion, between the baſe of the lamp and the ſurrounding oil, as indicated by the diſperſion of the duſt in the manner above deſcribed: and here the following conſiderations preſented themſelves:

The oil in the baſon, when of an uniform temperature, has all its parts in a ſtate of equilibrium and of reſt. When the lamp is lighted, it is evident we have a very active cauſe introduced tending to deſtroy that equilibrium. This cauſe is the flame which broods over a ſmall portion of the oil, and is ſeparated from it only by the intervention of a piece of paper or a wafer. The oil in ſuch circumſtances, in conſequence of being violently heated, muſt ſuddenly increaſe in volume, and muſt now, on account of the decreaſe of its ſpecific gravity, be preſſed upwards by a force ſufficient to raiſe part of it above the general level. But this heated portion of oil, in its endeavour to riſe up, will meet with a reſiſtance equal to the weight of the incumbent lamp, which will determine it, in ſeeking a vent, to ſlide out from under the baſe in a thin ſuperficial ſtream; and it ſeems to follow with equal certainty, that this conſtant ſtream will flow moſt rapidly and moſt copiouſly towards that ſide of the baſe of the lamp where the reſiſtance is leaſt, or where it has the ſhorteſt way to preſs forward; that is, from under the wick or flame to the edge of the baſe which is the neareſt, according to what we have ſeen to be agreeable to the phenomena. But, from the laws of motion, it is certain that the re-action of this ſtream of rarefied oil, thus iſſuing moſt rapidly and moſt copiouſly from a particular ſide of the baſe, muſt impel the lamp in the contrary direction, and make it ſail in the manner we have ſeen. It may further be remarked, that the heated oil ſo retreating from the flame, and endeavouring to riſe ſomewhat above the general level, in conſequence of its diminiſhed ſpecific gravity may more or leſs lift up that ſide of

of the base nearest the wick, and aid the re-action of the recoiling stream, by making the lamp sail in the opposite direction, as it were down hill.

That the rarefied oil under the base has really a constant tendency to rise above the general level, seems undeniable from the following facts; namely, that after any of the lamps has burned a little while, and has got its base soaked with the oil, as soon as the flame is blown out the lamp sinks to the bottom; and even a lamp with its base made of a thin lamina of talc sails very well till the flame is extinguished, and then it immediately sinks.

Agreeable to the explanation which has now been attempted, I found that when a topical heat was applied to the surface of the oil, by bringing the point of a poker dully red hot nearly into contact, there was soon produced a superficial stream or efflux from the iron in all directions, which cleared the face of the oil from the charcoal dust in a wider and a wider circle, till at last the whole particles were crowded together at the confines of the bason.

When the oil in this experiment was shallow, having gold leaf beat into very minute parts mixed with it, an opposite stream was observed below setting in towards the poker in all directions, and then rising upwards. But this general tendency of all the parts of the fluid, of moving in quest of an equilibrium, is illustrated in a very entertaining manner as follows: Into a tea cup or punch glass nearly filled with pure water, pour a dessert spoonful of very clear sallad oil with minute particles of gold leaf in it. If the water be cold, the oil when poured on at the centre, leisurely and continuedly, will rest upon the surface in the form of a lens, and remain insulated and equidistant from the sides of the vessel. A little lamp, when put upon this lens of oil and lighted, will sail and circulate as longer ones do in a bason. If it be now made to stand still, it is very amusing to observe the minute particles of the gold perpetually thrown out briskly at the stern in the superficial current, whilst the particles in the fund of the lens creep in all directions towards the lamp, and at last rise up under the base towards the flame, as the great centre of attraction, till they are caught by the retreating superficial stream, in which they rapidly trend off to some distance, when again they sink to renew the circulation.

When a patch of paper, or a wafer, or such light body, swims upon the oil in the bason, the point of a hot iron held near to it makes it flit its place, and move away by a seeming repulsion; but in reality by the heat generating a superficial stream flowing from the iron in all directions.

Again, if upon oil of turpentine, æther, alcohol, or any of the inflammable fluids possessing much tenuity, you throw a wafer much heated, it will immediately glide away and continue in motion till it cools; when the stream which issued from some part of it most copiously ceases. Double rum, melted tallow, bees-wax, and rosin, also afford the same continued efflux at the surface upon a topical application of heat, and the same phenomena as the oil does when little lamps are made to swim in them. It is somewhat remarkable, however, that though the inflammable fluids all agree in this, yet the topical application of heat at the surface of water does not produce similar effects.

For, if the point of a poker nearly red hot be held very close to the surface of water in a bason, the particles of the charcoal dust do not at all glide away, as they do in the case of oil, but seem to acquire only a slow irregular circular motion, which in time spreads wider,

whilst

whilst the floating motes, or particles of dust, keep nearly their relative places; and the same thing happens, though the point of the iron touches the water so as to make it simmer.

I do not well know how to account for this, unless it may be a consequence of the known much less expansibility of water by heat, compared to that of the inflammable fluids, and which may be so inconsiderable as not to destroy the equilibrium, so far as to produce an efflux from the lighter and expanded fluid immediately under the heated body. Possibly too the parts of the water, as soon as heated, may transmit the surplus temperature to the contiguous colder water, much more rapidly than the inflammable fluids do in like circumstances, and thereby resist the high temperature necessary to that degree of expansion which would disturb the equilibrium and produce an efflux; not to mention that the maximum of this temperature can never, at any rate, exceed 212 degrees, the boiling point of water.

That the equilibrium, however, amongst the parts of water is disturbed by the local application of heat, though in a much smaller degree than what obtains among the inflammable fluids, appears from an experiment I was led to make with a small thin cup swimming on water, and so contrived as to carry and feed with oil a wick placed a little way down from the lip in the inside, so as to be on a level with the water. The consequence of this construction was, that the cup moved upon the water very slowly, but always with the flame evidently sternmost. The same cup, when taken from the water and put into a bason of strong rum, sailed a great deal faster, and according to the same usual law.

I am much afraid that, by this time, I have wearied you by such a detail of minute facts and circumstances, and by those frequent repetitions which every new subject more or less requires.—And I ever remain, dear Sir, your most obedient faithful servant,

PAT. WILSON.

P. S. Should you be inclined to repeat any of the experiments, the following directions and miscellaneous observations may be attended to: The thread I made use of for the wicks was of that soft kind commonly employed in the flowering of muslin. After making the puncture in the base, you put through a bit of the thread, which clip short off below, and with a pin force in the burr gently round the thread, to give the base a proper hold of it. Then clip away the superfluous thread above, leaving the wick about a quarter of an inch long; and so the lamp is completed. Set it then upon the oil by taking hold of the wick, that the paper base may not be bent or distorted by handling it; and, after the wick is touched with a drop of oil, it is ready for being lighted. For this purpose a bit of packthread, which has been steeped in oil, is a cleanly and convenient match, and sheds no impurities on the oil, as a candle or wax taper would do.

When you want the lamp to circulate, the oil must be very pure, and brought into full contact with the sides of the glass. The oil and the bason, or salver, should all be allowed to come to the same temperature, between 55 and 60 degrees of Fahrenheit. For, if any part of the brim be much hotter than the rest, the lamp, on arriving there, will leave the side, by the current issuing from the heated part forcing it away.

Sometimes the lamp, when sailing, veers a little into a different direction, by the base altering or warping by the scorching heat of the flame, which determines the stream to flow out most copiously at a different part of the base.

In the melted greafe which lies round the wick of a common candle, when lighted, there are fometimes obferved atoms, which have been left by the fnuffers, moving to and from the flame continually. Thefe motions have been conceived by fome as occafioned by attractions and repulfions, in confequence of an electrical quality imputed to the flame. It fhould feem, however, that they depend merely upon oppofite currents, at the furface and immediately below the furface of the melted greafe, according to the principle above explained."

VII.

Experimental Refearches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. VENTURI, Profeffor of Experimental Philofophy at Modena, Member of the Italian Society of the Inftitute of Bologna, the Agrarian Society of Turin, &c.**

THE apparatus made ufe of in moft of the following experiments is the fame as that of Poleni †. It is reprefented at Fig. 1. Pl. VIII. The refervoir X, of a conical form, has forty inches diameter at CE, and 30 at OP. FP is a broad plate of copper, the plane of which is perpendicular to the horizon; it is applied to the infide of the refervoir. The valve or flap FS, moveable by the handle K, is drawn up againft the fide of the veffel above F, in order that it may not impede the courfe of the particles of the fluid contained in the refervoir to the aperture P. I have applied different ajutages to this aperture, according to the exigence of the cafe. The tubes which I applied were made of tinned iron of the beft quality; the longitudinal junction of the edges was made by immediate contact, and not by overlapping, and the whole of the workmanfhip was executed with great care. When the aperture was fimply a hole through a thin plate, the thicknefs of its edge did not exceed one fourth of a line.

The upper veffel Z ferves to maintain the water of the refervoir X at the conftant height of the line CE, while it flows out through P. The plug AB is drawn more or lefs back, in order to regulate the introduction of the fupply. The box or fhelf DL prevents this water from exciting by its fall any agitation which might influence the emiffion at P. The opening at Q difcharges the fuperfluous water which might rife above the line CE. The height of the furface CE above the centre of the orifice at P was 32,5 inches, in all cafes where it is not otherwife expreffed.

Moft of the experiments here defcribed were made in public at the Philofophical Theatre of Modena; various men of fcience were prefent at the reft; the different departments of experiment were performed by feveral perfons at the fame time. One of thefe operators repeated the feconds audibly from the clock; another drew back the valve SF; a third regulated by the means of the plug B the introduction of the fupply of water, fo that a very thin

* Since the receipt of this memoir (which is equally valuable as the fource of data for fcientific proceffes, and of ufeful practical refults, either unknown or difregarded in hydraulic works) it has remained in my hands with the intention of prefenting my readers with an abftract of its contents. But, upon ftricter confideration, I have thought it beft to give it entire from the French original. N.

† De Caftellis. This treatife is reprinted in the third volume of Hydraulic Treatifes, publifhed at Parma. V.

ſheet of water conſtantly flowed at Q. At the inſtant agreed upon, the paſſages of the water were again cloſed. Every experiment was repeated ſucceſſively for a number of times, until the agreement of the reſults had removed every ſuſpicion of error. I am aſſured that even in the moſt complicated caſes, the quantity of error could not exceed one fortieth part.

The meaſures indicated in the courſe of theſe experiments were taken from a toiſe adjuſted by that of the Academy, which Citizen Lalande ſent me in 1783. Theſe meaſures, as well as all the others of the 18th century, will undergo the fate which is prepared for them by the eſtabliſhment of the new metre. They may be reduced to this new ſtandard, by obſerving that the foot is to the metre as 100 to 308.

The wiſeſt philoſophers have their doubts with regard to every abſtract theory concerning the motion of fluids; and even the greateſt geometers avow, that theſe methods, which have afforded them ſuch ſurpriſing advances in the mechanics of ſolid bodies, do not afford any concluſions with regard to hydraulics but ſuch as are too general and uncertain for the greater number of particular caſes. Impreſſed with a conviction of this truth, I have attended to theory only when it combined with the facts, and was neceſſary to unite them under a ſingle point of view. Even this ſmall portion of theory may, if the reader pleaſes, be rejected; and he may conſider the following propoſitions ſimply as the reſults of experiment.

When I quote the eſtimable work of Citizen Boſſut, on hydrodynamics, I refer to the edition of 1786*.

Propoſition 1. The motion of a fluid is communicated to the lateral parts which are at reſt.

Newton has affirmed, that when motion is propagated in a fluid, and has arrived beyond the aperture B C, fig. 2, the motion diverges from that opening, as from a centre, and is propagated in right lines towards the lateral parts N K, as well as towards S. The ſimple and immediate application of this theorem cannot be applied to a jet which iſſues from the aperture B C at the ſurface of ſtill water. Circumſtances enter into this caſe, which tranſform the reſult of the principle into particular motions. It is nevertheleſs true, that the jet B C communicates its motion to the lateral parts N K; but it does not repel them towards P and Q, but on the contrary tranſports them along with its own ſteam towards S.

Experiment 1. The horizontal cylindric pipe A C, fig. 3, is introduced into the veſſel D E F B, which is filled with water as high as D B. Oppoſite and at a ſmall interval from the aperture C commences a ſmall rectangular channel of tinned iron, S M B R, which is open at top S R; the inclined bottom M B reſts on the edge of the veſſel B. It is 24 lines broad; the diameter of the tube A C is 14,5 lines; the extremity A is applied to the aperture P of fig. 1. The water of the reſervoir being ſuffered to flow through the tube A C, the jet riſes along the ſmall channel M B, and flies out of the veſſel in the ſtream B V. By this means a current is produced in the fluid of the veſſel D E F B; this fluid enters into the channel S R, and iſſues by M B V along with the jet A C, ſo that in a few ſeconds the water D B falls to M H.

* I conſider this treatiſe as ſuperior to all which before were extant. It is founded on a combination of the principles of experiment and of theory. I have profited by theſe principles, and ſeveral particular remarks which the ſame Citizen Boſſut and Citizen Prony have been ſo good as to communicate after peruſal of my memoir. V.

Experiment

Experiment 2. Bring ſome very light or moveable bodies near the jet of water P Y, fig. 1, which iſſues from the aperture P, and falls from a certain height E into the inferior veſſel R T. It is ſeen that theſe bodies are carried along by the air which deſcends with the jet P Y. Part of this air is carried along and plunged into the water of the inferior veſſel.

Theſe experiments clearly prove, that the fluid which iſſues by B C, fig. 2, impreſſes its motion on the lateral parts N K; not by impelling them towards P Q, but by carrying them along with itſelf towards S. I call this the lateral communication of motion in fluids. Newton was acquainted with this communication, and has deduced from it the propagation of rotatory motion from the interior to the exterior ſtrata of a whirlpool. Is this lateral communication of motion occaſioned by the viſcidity or mutual adheſion of the parts of the fluid, or their mutual engagement or intermixture, or the divergency of thoſe parts which are in motion? We may perhaps be able to give ſome account of this when we ſhall have ſeen the effects; but in the mean time, whatever may be the cauſe, let us take the effect as experience points it out; let us conſider it as a principle, and endeavour to apply it to ſome particular caſes in order to aſcertain the reſult.

The firſt circumſtance to which I propoſe to apply this principle is the increaſe of expenditure of fluid iſſuing out of an orifice fitted with additional tubes.

Propoſition 2. If that part of an additional cylindric tube which is neareſt the ſide of the reſervoir be contracted according to the form of the contracted vein of fluid which iſſues through a hole of the ſame diameter in a thin plate, the expenditure will be the ſame as if the tube were not contracted at all. It is well known, that when the water of a reſervoir is ſuffered to flow through a circular orifice in a thin plate, the fluid vein which forms the jet becomes contracted at a ſhort diſtance from the orifice; and the diameter of the contracted vein is nearly 0,8 of the diameter of the orifice. Poleni firſt obſerved, that by applying an additional cylindric pipe to the orifice, of the ſame diameter as the orifice itſelf, and from two to four times that length, the expenditure is increaſed from 100 to 133. To account for this augmentation, he ſuppoſes that the fluid vein is leſs contracted in pipes than after paſſing through the thin plate. The ſuppoſition was not unreaſonable; but it could not apply to the caſe announced in this propoſition. I ſhall proceed to give the particulars in the following experiment.

Experiment 3. To the aperture P, of fig. 1, I applied a circular orifice 18 lines in diameter, pierced through a thin plate. Four cubical feet of water flowed into the veſſel Y in 41 ſeconds.

I then applied to the orifice a cylindric tube of the ſame diameter, and fifty-four lines long. The four cubic feet flowed out in 31 ſeconds.

Inſtead of this ſimple cylindric tube, I applied the compound tube of fig. 5; the parts of which have the following dimenſions in lines: $AC = GI = MN = 18$; $DF = 14,5$; $AB = 11$; $BG = 10$; $GM = 37$; $AM = 58$. With this compound tube the expenditure of four cubic feet of water was made in 31 ſeconds, as with the ſimple cylindric tube.

The form of the conical portion A C D F was nearly the ſame as that of the contraction of the vein which iſſues through a thin plate. The vein muſt therefore have paſſed through a contraction nearly equal to that of the contracted vein from a thin plate; the expenditure never-

neverthelefs was more abundant, in the fame proportion as through the fimple cylindric tube. It follows, therefore, that the velocity of the fection D F, and of the whole conoid A C D F, muft have been greater than that of the contracted vein from a thin plate; and it remains to be fhewn what was the caufe of this augmentation of velocity which takes place within the tube, and does not manifeft itfelf externally.

That the conical tube A C D F does not itfelf caufe any augmentation of expenditure, is evinced by the following.

Experiment 4. The conical tube A C D F, from which the remaining part D G M N I F was feparated, was applied to the orifice P. The four cubic feet were emitted in 42 feconds, which is the time of the expence through the orifice itfelf A C in the thin plate, with the difference of one fecond only. This flight variation arifes from its being almoft impoffible to make the tube A D C F perfectly of the form of the natural contracted vein.

Propofition 3. The preffure of the atmofphere increafes the expence of water through a fimple cylindric tube, when compared with that which iffues through a hole in a thin plate, whatever may be the direction of the tube.

It has long been known, that a heavy fluid which moves in a defcending cylindric pipe tends to accelerate its motion. The inferior parts tend to feparate themfelves from the fuperior, and by that means caufe the preffure of the atmofphere to increafe the velocity of the fuperior parts. This fucceffive acceleration of gravity cannot take place in an horizontal or afcending pipe. We fhall neverthelefs find that the preffure of the atmofphere acts even in thefe laft fituations to increafe the velocity of fluid within the pipe. Certain queftions of legal right, which arofe in my country, refpecting the quantity of water fupplied by a pipe for watering lands (canal d'arrofement) directed my attention to this object.

In the year 1791 I made the following experiments publicly in the Theatre of Natural Philofophy at Modena:

Experiment 5. To the aperture P, fig. 1, I applied a cylindrical pipe 54 lines in length and 18 in diameter. At the diftance of nine lines from the interior orifice P, twelve fmall holes were made in its circumference. When thefe fmall holes were open, the four cubic feet iffued out in 41 feconds, in the fame manner as through a thin plate. Not a fingle drop paffed through any of the holes, and the ftream did not fill the tube. The holes were then clofed one after the other with wet fkin. As long as there was one hole open the expence continued the fame; but when at laft all the twelve holes were well clofed, the fluid ftream iffued out in a body which filled the pipe, and the four cubic feet were emitted in 31 feconds.

Experiment 6. To the cylindric tube K L B, fig. 6, 18 lines in diameter and 57 lines long, was joined the glafs tube Q R S T, at the diftance of eight lines from the interior orifice K. The glafs tube was plunged in coloured water contained in the veffel T. When this apparatus was applied to the aperture P, fig. 1, the four cubic feet of water flowed out in 31 feconds. The coloured liquid T rofe in the tube T R as high as S, at the height of 24 inches above the furface T.

The branch R T of the glafs tube was fhortened fo that R T was only fix inches longer than R Q. The efflux being then permitted to take place, the coloured liquor of the veffel T rofe through the tube R T, and mixed with the water which flowed from the refervoir through K V, both of which flowed out at V, and in a fhort time the veffel T was emptied.

I repeated

I repeated this experiment with the compound tube fig. 5, and the results were the same.

Experiment 7. The cylindrical pipe K L V, fig. 6, was applied in an ascending and nearly vertical situation to the orifice R, fig. 8, of the vessel H I, of which the end H communicated by an opening of considerable extent with the water of the reservoir X, fig. 1. The charge on the upper extremity V of the tube was 27,5 inches. I inclined the tube a little from the vertical direction, in order that the jet might not fall back upon itself. The glass tube Q R T, fig. 6, in this new situation was so disposed that its lower extremity was immersed as before in the coloured liquid of the vessel T. When the efflux was permitted, the expenditure of four cubic feet was made in 34 seconds; and the coloured liquid rose in the tube R T to the height of near 20 inches. With the same charge of 27,5 inches the orifice of 18 lines in a thin plate would have afforded the four cubic feet in 45 seconds.

Experiment 8. A cylindrical vessel of 4,5 inches diameter had in its vertical sides near the base a circular opening of 4,5 lines in diameter, opened in a thin plate of tinned iron. The surface of the water contained in this vessel was 8,3 inches above the centre of the aperture. The water was then suffered to flow out of this aperture in the thin plate, and its surface was depressed seven inches in the vessel in 27,5 seconds of time.

To the same aperture was applied a cylindric tube of the same diameter, and in length 11 lines. The vessel was filled to the same height as before, and, the water being suffered to flow out, its surface was depressed seven inches in 21 seconds.

The same experiment was afterwards repeated in the receiver of the air-pump, under which the mercurial gauge stood at no more than 10 lines in height. The surface of the water in the vessel was depressed seven inches in 27,5 seconds, whether the aperture was made in a thin plate, or whether it was provided with an additional cylindric tube.

The height of the coloured water in the tube of glass measures the active quantity of the pressure of the atmosphere, which is exerted on the surface of the water to increase the expenditure. For example, in the sixth experiment 32,5 + 24 inches charge on the orifice P, and we have nearly $\sqrt{32{,}5} : \sqrt{56{,}5} :: 31'' : 41''$, as is required by the common theory of the motion of fluids which issue out of vessels by a small aperture. The same obtains in Experiment 7.

Daniel Bernoulli made the 7th experiment in descending tubes, and in diverging conical tubes, and explained the result merely by the theory of conservation of living forces. Euler and d'Alembert observed to him, that the pressure of the atmosphere was concerned in the effect*. Though the case of the descending tube be different from that of the horizontal or ascending tube, the knowledge of the first of these two cases may nevertheless facilitate the knowledge of the second. Besides which, the causes which act in both cases are often combined together, and it is necessary to be well acquainted with both, in order to distinguish the results. On this account it is, that in the following proposition I have turned from my principal subject for a moment to consider the first case, after which I shall return to the second.

Proposition 4. In descending cylindrical tubes, the upper ends of which possess the form of the contracted vein, the expence is such as corresponds with the height of the fluid above the inferior extremity of the tube.

* D'Alembert, Traité des Fluides, Art. 149.

The

The ancients remarked, that a descending tube applied to a reservoir increases the expenditure*. Mariotte estimated that the water issues through C Q, fig. 7, with a velocity nearly the mean proportional between the velocities arising from the two heights A B, A C†. Guillielmini sought for the cause of this augmentation in the weight of the atmosphere, and determined the velocity at C to be the same as would arise from the whole height A C‡. In his reasoning he supposes that the pressure at C is the same for the state of motion as for that of rest; which is not true. In the experiments he made upon this object, he paid no regard either to the diminution of expenditure produced by the irregularity of the inner surface of the tubes, nor the augmentation occasioned by the form of the tubes themselves. By a singular accidental concurrence, one of these errors compensated for the other. I know of no other decisive experiment on this head since Guillielmini. I shall, therefore, proceed to establish the proposition upon the principle of virtual ascension combined with the pressure of the atmosphere, and that in a manner which shall be clear of every objection, of theory as well as of experiment.

Let B L K O represent a conical tube adapted to the form of the contracted vein §; the cylindrical tube L C Q K is of the same diameter as the contracted part. The fluid stratum, L K, continuing to descend through L C, tends to accelerate its motion, according to the laws of gravitation; and consequently when it passes from L K to M N, it tends to detach itself from the stratum which follows, or in other words it tends to produce a vacuum between L K and M N; and the same effect takes place through the whole length of the tube L C. The pressure of the atmosphere becomes active as far as is necessary to prevent the vacuum; and its action is alike both at the surface of the fluid at A, and at the inferior extremity of the tube at C. At A it increases the expenditure, and at C it destroys the sum of the accelerations which would be produced along L C, so that the fluid remains continuous in the tube.

Let T represent the time which the continuous column of fluid L C Q K employs to pass through the tube L C, whatever may be the velocity at L, and the successive acceleration from L to C. And if we suppose this same column to return upwards from D to E, it will pass through the space D E = L C in the same time T; during which it will lose all the acceleration it acquired from L to C. The pressure of the column E D, continued for the time T, is therefore the quantity required to destroy the successive acceleration from L to C, and to prevent the fluid from ceasing to be continuous in the tube L C: consequently that part of the pressure of the atmosphere which is exerted at C Q to destroy the sum of the accelerations through L C, is equal to the pressure of a column E D of a fluid, homogeneous to that of the reservoir A B. And since the same pressure must also be exerted on the surface A of the reservoir, if we take F A = L C, the fluid at L K will possess the velocity which is proper to the height F L = A C; without considering the retardation which the external inequalities of the tube L C Q K must produce.

* Calix devexus amplius rapit. Frontin. de aquæduct. Art. 36. See also the Pneumatics of Hero, in the mathem. vet. ed. 1693, page 157.

† Mouvement des eaux, part. 3, disc. 2.

‡ Epist. hydrostatic. Oper. tom. 1. page 212.

§ When I speak of the form of the contracted vein, I always mean to express the conoid formed by the fluid issuing from an orifice through a thin plate.

Experiment IX. 1. The orifice P (fig. 1) through a thin plate is circular, and 18 lines in diameter. The charge of fluid above the centre of the orifice is 40 inches. Four cubic feet of water were emitted in 38 seconds.

2. To the orifice P, fig. 1, I applied the tube A C D, fig. 4, the upper end of which A C had the form of the contracted vein. The diameter at A was 18 lines in length, A D 31 inches, and the situation of the tube horizontal. The expenditure of four cubical feet was made in 48 seconds.

3. The same orifice and the same tube were applied to the horizontal bottom of the reservoir fig. 7, so that the tube was vertical, and A C = 40 inches, or the height of the charge in the two former experiments. The four cubic feet flowed out in 48 seconds, as in the second experiment.

Experiment X. The last described experiment was repeated with a circular aperture of 11,2 lines in diameter. The extremity A C, of the tube fig. 4, had the form of the contracted vein; the end A having the same diameter as that of the orifice. The other circumstances were as in the preceding cases. In the disposition, according to the first case four cubical feet of water flowed out in 98 seconds; in the second case the time was 130 seconds; and in the third case 129 seconds.

In each of these two experiments the tubes and the expence of water were the same for the second and the third cases; whence it follows, that the force by which the expenditure was governed was the same in both cases. Now the force which acts in the second case is the same as in the first; and consequently the same force likewise acts in the first and third cases. All the difference of the result between the first case and the two following arises from the retardation produced by the inequalities of the internal surface of the tubes.

Experiment XI. The height A B, fig. 7, being constantly 32,5 inches, and the orifice B O 18 lines in diameter, the tube B O C Q was applied to the orifice itself, the superior extremity of this tube having the form of the contracted vein. When the length of the tube was varied, the times of the efflux of four cubic feet of water were as in the following table.

Length of the tube B C in inches.	Time of efflux of four cubic feet by experiment.	Time according to the theory without considering the retardation.	Difference between the theory and experiment.	Retardations computed from the following experiment.
3	41″	40″	1″	1″,3
12	38″	35″,2	2″,8	3″,4
24	35″	31″,2	3″,8	5″.

The fifth column of this table is calculated from the proportion of retardation produced by the irregularities of the internal surface of the tubes. Citizen Bossut has observed, that these retardations* increase rather in a less ratio than the velocity of the stream. This is perhaps the reason of the difference observed between the fourth and fifth columns.

Experiment XII. I applied to the orifice P, fig. 1, the same tubes as in the foregoing experiment one after another in an horizontal situation, the height of the charge being constant-

* Hydrodyn. Art. 622.

ly

ly 32,5 inches above the centre of the orifice. The times of emiſſion were as in the following table.

Length of the tube B C in inches.	Time of efflux of four cubic feet.	Differences.
0	41″	0
3	42″,5	1″,5
12	45″	4″
24	48″	7″

I muſt here obſerve, that the viſcidity or mutual adheſion of the particles of the water * is of very little conſequence to the increaſe of expenditure through the orifice B O, fig. 7, by the additional tube B C. For as ſoon as a ſmall hole is opened at K the increaſe of expenditure diminiſhes or entirely ceaſes, and the fluid is no longer continuous in the tube.

We will now return to tubes in the horizontal and aſcending ſituations.

[*To be continued.*]

VIII.

Experiments and Obſervations on the Effect of Annealing a Plate of Metal, conſiſting of fine or alloyed Gold, with one twenty-fourth Part of Tin. By MATTHEW TILLET.

[Concluded from page 142.]

I HESITATED to uſe my gold of 24 carats, from the juſt fear I had that it would loſe the advantage of being the ductile of metals, and that I could not reſtore this property but by the method of parting. For this reaſon I determined to begin my experiments on a more deciſive ſcale than the former, upon gold of 22 carats, or alloyed with $\frac{1}{12}$th part of copper. In this manner I ſhould repeat ſome of the experiments related by Mr. Alchorne, from which he has conſtantly inferred that tin, mixed in a certain proportion with gold of this ſame fineneſs of 22 carats, does not deprive it of its ductility.

I therefore alloyed 1 gros 24 grains of tin taken from the ingot deprived of arſenic, which I have mentioned, with 4 ounces of gold, the fineneſs of which, namely 22 carats, was perfectly well aſcertained. Theſe two metals, reduced into ſmall pieces, were mixed together, put into a crucible, and urged by the ſtrong heat of a forge with two pair of bellows. When their fuſion appeared to be complete, I poured the metal into a ſmall ingot-mould proportioned to the quantity.

The ingot thus obtained had loſt ſcarcely any thing of the weight of the two metals that compoſed it, which was a proof that the tin had united and incorporated with the four ounces of gold. But on attempting to bend the ingot, which was about ſix inches long and not more than two or three lines thick, I remarked, contrary to the nature of gold of 22 carats, that it was rigid, and would have required a conſiderable effort to give it any degree of

* Graveſande and others have attributed the increaſe of expenditure through deſcending tubes, to the natural coheſion of the particles of water. V.

of curvature, or bring it to the flexibility it would have poſſeſſed if no tin had entered into its compoſition. Hence I clearly ſaw, that this rigidity announced a diminution of its ductility; that the interpoſition of a ſubſtance, which was foreign to the gold, and incapable by its nature of maintaining the coheſion, was the cauſe of this want of flexibility; and that it could be attributed only to the tin, becauſe copper alone, mixed with fine gold, though it gives a greater degree of hardneſs and rigidity than it before poſſeſſed, deprives it of very little of its ductility.

After this firſt obſervation on the ſtate of the ingot afforded by the experiment, I came to the more deciſive proof by hammering, particularly with the edge of the hammer, in order that the bar might be lengthened, and by that means ſubmitted to the moſt deciſive proof. I did not obſerve during the continuation of this proceſs, till the bar was reduced to about two-thirds of its firſt thickneſs—I did not obſerve, I ſay, that its edges were cracked, or exhibited much of the appearance of brittleneſs; but as I was apprehenſive that this accident might happen by too long hammering, I divided the bar by cutting off the part which had been hammered out. This part was placed in the midſt of lighted charcoal, in order that, by a moderate annealing, it might recover the ſtate of malleability it poſſeſſed before it was hammered.

But when I went to take it out of the fire, where it had undergone no greater heat than a cherry-red, I found it divided into two parts. After having ſuffered theſe to cool, I forged them again. They were extended with conſiderable eaſe, though with ſome cracks at the edges; but they did not yet ſatisfy the whole of my enquiries. I therefore annealed one of the two laſt-mentioned pieces a ſecond time, and reſerved the other in its hard hammered ſtate to be paſſed between the laminating rollers. The annealed part, which might have the thickneſs of about a ſhilling (piece de douze ſols), broke in the fire, though the heat was very gentle, into four or five portions. The longeſt of theſe portions, which beſt reſiſted the action of the fire, bent and twiſted itſelf, and ſhewed, by this ſtate of ſtrong contraction in different directions, that it had tended to break and become divided into ſmall portions ſimilar to thoſe which had already ſeparated from it.

This accident gave me reaſon to ſuſpect that the aſhes, upon which I had annealed the plate of gold which broke in pieces, might alſo contain certain portions. I was not deceived; for the aſhes being carefully waſhed left three or four fragments which the ignited coals had prevented from being ſeen.

It has been remarked, that I reſerved one of the two parts of the portion of the ingot which I had forged a ſecond time, and that I had kept it in its hammer-hardened ſtate. I could not doubt, after the laſt-recited experiment, that any attempt to anneal this portion, even by the moſt moderate heat, would be attended with the ſame conſequence, namely, that it would break in pieces. I therefore determined to extend it ſtill more between the rollers, ſetting them up very gradually in order that the fracture, if it ſhould take place, might be principally owing to the brittleneſs of the material, and not to the force of compreſſion to which it was ſubjected. By this management I ſucceeded in extending the metal to double its length notwithſtanding its hardneſs. In this manner it was rendered as thin as ſtrong paper; though it muſt be confeſſed that the edges were cracked through their whole length like the teeth of a ſaw. But this accident is not at all ſurpriſing, when it is conſidered that gold, though alloyed ſimply with copper, whatever may be the cauſe,

does

does not possess its usual ductility, particularly when it is laminated very thin without repeated annealing as the metal becomes hard.

It might be presumed, in reflecting on the experiment I have related, that the fracture of the pieces of gold was owing to an incomplete fusion or unequal mixture of the two metals. I was aware, from this notion, that it was proper to melt the ingot over again with all the parts which had been separated from it, and to neglect no precaution, after well mixing it, that it should be poured out in perfect fusion. At the moment this state of fusion was obtained, I threw a small quantity of calcined borax upon the metal, in order that its surface might become clear, and every foreign substance might be carried to the circumference. No other consequence could arise from this use of borax than a greater softness in the compound, and consequently less risk of its breaking when it came to be subjected to the hammer.

All my precautions were useless. I forged one end, which was lengthened very well without any perceptible crack; but the extremity of this ingot, afterwards reduced to a small thickness and exposed to the annealing heat, became divided into several parts, the longest of which was contorted, and would no doubt have broken if the heat had been stronger, or continued for a greater length of time.

Though the experiment, of which I have related the result, on the mixture of one part of tin and 24 parts of gold of 22 carats, might, in strictness, have been sufficient to prove that this alloy deprives gold of great part of its ductility, and exposes it, while annealing, to an accident which artists would scarcely be able to avoid, from the necessity they are under of continually restoring the malleability of the gold they work, by annealing it after it has become hard under the hammer; I nevertheless thought it proper to lay aside the objection I had to deprive my fine gold of its ductility, which could not be restored but by a new parting essay, and to repeat the experiment by using gold of 24 carats in such a quantity that my operation might be compared with that of Mr. Alchorne, from which chiefly he has drawn his conclusion.

I therefore employed 6 ounces of fine gold and 2 gros of tin for this new experiment. The first of these metals was divided into a great number of pieces, and I had included the two gros of tin in two leaves of gold taken from the 6 ounces, rolled out very thin, and so flexible, after annealing, that they perfectly enveloped the tin. After having put one part of the gold at the bottom of a small crucible, I placed the tin wrapped up in gold upon it, and over this I put the remainder of the gold.

When the whole of the two metals was in perfect fusion, I poured them speedily into an ingot mould, which I had before used, and obtained an ingot rather longer and cleaner than the two former.

As soon as it was cold, I forged one of its extremities with the edge of the hammer. It was lengthened without any perceptible crack; and when it was reduced to the thickness of one line, or thereabouts, I cut it off for separate treatment. By moderate annealing it maintained its integrity; and, with the exception of a few cracks, it passed the laminating rollers without breaking. As I was fearful, nevertheless, that it might break in some part if I continued to laminate it, I gave it a slight annealing. It had scarcely acquired a cherry redness between the charcoal before it broke into five or six parts, some of which were simply bended or twisted, and others flat as they quitted the rollers. Among the annealed

 pieces

pieces of this extremity of the ingot, there was one sufficiently long, though a little curled, which I laminated a second time, with the determination of rendering it very thin without the least annealing. It acquired at least double the length it had at first without breaking; and, if we except the two sides of this plate which were cracked, the body, or main piece, was entire. It was spongy, and might be considered as if formed out of an ingot of common gold containing no tin, but not possessing the whole of its natural ductility.

It follows, from these experiments, that gold, whether fine or alloyed, when perfectly fused with a small portion of the finest tin, acquires rigidity and hardness by the mixture; that it loses somewhat of its distinguishing colour; and that it may, indeed, by careful management, be extended to a certain degree by the hammer, or still better by the rollers; but that, as it cannot be annealed without danger of breaking, it is by this defect deprived of the essential advantage of recovering its original softness after it has been strongly hammer hardened. It is not but by careful management in the use of the hammer, and by frequent annealing, that artists employed on works of gold and silver succeed in obtaining them without cracks, and bringing them to a state of perfection, without being obliged to have recourse to solder to repair the defects which excessive hardness under the hammer would occasion. How much, therefore, ought gold-workers, who continually have this metal in their hands, to be attentive to prevent the introduction of tin in their workshops, and never to employ such compounds of gold as are subject to break, or even to warp, while annealing! The expence of refining, which they would pay for depurating such compounds, would be of less consequence to them than the loss of time required for the careful management of such gold contaminated by tin, even if they did succeed in using it, and were not often forced to abandon, after much labour, a work nearly finished.

I do not doubt but that Mr. Alchorne, if he had carried his experiments further, or had considered them with regard to the methods of gold-workers, who frequently expose to the fire such pieces as they propose to raise or fashion according to their designs;—I do not doubt, I say, but that Mr. Alchorne would have cautioned artists against the accidents to which gold alloyed with tin is subject while annealing. He has observed a degree of ductility in this matter, and has not presumed that it might be taken away by means of fire, which, on the contrary, restores to most metals their flexibility and facility of working.

The opinion which has hitherto been maintained respecting the danger of a very small alloy of tin with gold is therefore well founded. In fact, it must have been difficult for such an opinion to have prevailed without foundation, when a multitude of artists are in a situation to verify the fact, and must be immediately struck with the brittleness of such gold, and be most strongly interested to discover its cause.

This example of an opinion generally received and supported on constant facts, which the greater number of artists have rather adopted than examined; this example proves that we ought not, without the greatest caution, to attack such received notions, particularly when as in the present case it is in no respect hurtful, and only tends to render the workmen more cautious in the use of the most valuable of metals.

The experiments of Mr. Alchorne have long remained upon record in the Philosophical Transactions, and have by that means acquired a sanction which demands a greater degree of attention. I have had no other aim in repeating them, and exhibiting an essential dependant fact not mentioned by that skilful assayer, than to give useful information to artists,

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and contribute to the certainty of their operations. I think this purpose will be answered, by leaving them in possession of all the apprehension they have hitherto entertained respecting the mixture of tin with gold.

If it should be thought that my experiments may not be as conclusive as at first sight they appear, they will at least produce no other effect than that of giving artists some useless trouble. They will with reason attend to them, however superfluous they may be supposed, and will prefer the care I recommend to the anxiety of working upon materials rendered suspicious by their harshness, and supposed to be incapable of annealing on account of their containing tin.

If it be allowable to form some conjectures on the fracture of plates of gold containing tin, when subjected to the annealing heat, it may be presumed, that the tin, which very speedily melts, while the gold requires a very strong heat for its fusion—it may be presumed, I say, that the parts of the tin intermixed in a sort of proportional equality with those of the gold, tend to separate by a speedy fusion and at a very gentle degree of heat; that they remain without consistence between the parts of the gold, while the latter preserve the whole of their solidity, and do not lose it even by the annealing heat; whence it seems, that the parts of the precious metal, when ignited among the coals, having no longer the solid connection formed by the tin, but, on the contrary, having an infinite number of small cavities occupied by particles of that metal in fusion, must tend to disunion; whereas the same accident does not take place in the pieces which have resisted the annealing, and have been laminated after cooling, because the particles of tin have become solid by cooling, and have recovered their original state of union with the gold.

This fracture of the compound does not take place with an alloy of gold and copper, for an opposite reason to that which has here been explained; namely, because these two metals require nearly the same heat for their fusion. The effect of annealing being therefore equal upon both, the metals, notwithstanding this treatment, preserve their natural consistence, even though the heat be carried near the point of fusion.

In support of the opinion I have presented, respecting the fracture of these plates of gold, I must observe that, by examining their surfaces under the microscope, a great number of particles of tin may be distinguished, which appear to be disengaged from the pores of the gold; that these surfaces, being applied by an annealing heat to a plate of iron or silver, adhere strongly by virtue of these particles of tin; that they cannot be separated but with difficulty, and even tear up some slight portions of the metal on which they were annealed when they are so separated.

I shall conclude these details, into which the subject of my experiments has forced me to enter, by remarking that the plates of gold here spoken of may be kept entire, when annealed, on a flat plate of metal, on which they may remain supported till cold; whereas, if placed on ignited charcoal, they frequently bend or twist, and are subject to break, particularly if an attempt is made to take them out with the tongs in their ignited state.

IX.

On the Knowledge of the Ancients respecting Gravity. By a Correspondent.

THE respectable publishers of the "Journal of Natural Philosophy, Chemistry and the Arts," are desired to submit to the inspection of its ingenious Author the following observations relative to a Note at page 85 of the 15th Number. (Vol. II.)

"In the treatise of 'Plutarch De Placitis Philosophorum,' occurs no passage referring to the 'vibration of a body through the centre of the earth to the antipodes, and to the retention of the moon in its orbit by the combination of the projectile and gravitating forces.'"

The passage alluded to is probably in the treatise "De Facie quæ in Orbe Lunæ apparet;" but there, so far from proving a gravitating force, the absurdity of the supposition is endeavoured to be shewn.

Ed. Oxon.—8vo. T. iv. § ζ. *Φιλοσοφων δε ουκ ακουστεον, αν τα παραδοξα παραδοξοις αμυνεσθαι βουλωνται, και μαχομενοι προς τα θαυμασια των δογματων, ατοπωτερα και θαυμασιωτερα πλαττωσιν· ωσπερ ουτοι την επι το μεσον φοραν εισαγουσιν. Ἡ, τι παραδοξον ουκ ενεστιν; Ουχι την γην σφαιραν ειναι, τηλικαυτα βαθη και υψη και ανωμαλιας εχουσαν; Ουκ αντιποδας οικειν, ωσπερ θριπας, η γαλεωτας, τραπεντα ανω τα κατω τη γη προϊσχομενους; ημας δε αυτους μη προς ορθας βεβηκοτας, αλλα πλαγιους επιμενειν απονευοντας, ωσπερ οι μεθυοντες; Ου μυδρους χιλιοταλαντους δια βαθους της Γης φερομενους, οταν εξικωνται προς το μεσον, ιστασθαι μηδενος απαντωντος, μηδε υπερειδοντος; Ει δε ρυμη κατω φερομενοι το μεσον υπερβαλλοιεν, αυθις οπισω στρεφεσθαι και ανακαμπτειν απ' αυτων.* Thus translated by Philemon Holland—"But we ought not to give ear unto philosophers, if they would maintain strange "paradoxes by other positions as absurd, or, to confute admirable opinions, devise others "much more extravagant and wonderful, like as these here who broach and bring in a "notion, forsooth, tending unto a middle, wherein, what absurdity is there not? Hold "not they that the earth is as round as a ball, and yet we see how many deep profundities, "haughty sublimities and manifold inequalities it hath? Affirm not they that there be an-"tipodes dwelling opposite one unto another, and those sticking as it were to the sides of the "earth with their heels upward and their head downwards topsy turvy, like unto these wood-"worms or cats (r. newts) which hang by their sharp claws? Would not they have even us also "that are here, for to go upon the ground not plumb upright, but bending or inclining side-"long, reeling and staggering like drunken folk? Do they not tell us tales, and would make "us believe, that if bars and masses of iron weighing a thousand talents a piece were let fall "down into the bottom of the earth, when they came once to the middle centre thereof, "will stay and rest there, albeit nothing else came against them nor sustained them up? And "if peradventure by some forcible violence they should pass beyond the said midst, they "would soon *rebound back thither* again of their own accord?"

So true is the observation of Cleomedes, that those among the ancients who treated of natural philosophy were much confused, and on this head greatly erred, not being able to discern, that since the world was of a spherical figure, the centre must of necessity be inferior to every part of it—*αι δε λοιπαι σχεσε πολλην παρεσχον ταραχην τοις παλαιοτεροις των φυσικων—ου συνηθεντων επιστησαι, οτι εν τῳ κοσμῳ, σφαιρικῳ τῳ σχηματι οντι, κατω μεν απο παντος αυτου, το μεσαιτατον ειναι αναγκαιον.* De Meteoris, ed. Balforei, 1605, p. 9.

Remarks

Remarks on the preceding Communication.

UPON consulting the original, to which I referred by memory, in the note at page 85 of the present volume, I perceive that both the passages alluded to are in the treatise De Facie in Orbe Lunæ. I was misled by another obscure passage in the third book De Placitis Philosophorum, under the title Περι κινησεως γης. ιγ. where it is said that Philolaus the Pythagorean held that the earth revolves in an orbit (περι το πυρ) around the fire, and that Heraclides of Pontus and Ecphantus the Pythagorean attributed motion to the earth, not progressive, but rotatory, from west to east. I did not, as my learned correspondent seems to intimate, pretend to say that either Plutarch or the speakers in his treatise exhibited the doctrines of the gravitating and projectile forces, as being worthy of adoption. My implied argument was, that since they are extant in a passage of some length, there were in fact philosophers among the ancients who had maintained and developed them; though, from a variety of well-known causes, this doctrine remained without distinction among other ill-digested or false systems. The combination of these forces in the moon, which occurs in the paragraph to which Theo replies, in my correspondent's quotation, is καιτοι τη μεν σεληνη βοηθεια προς το μη πεσειν, η κινησις αυτη και το ριζωδες της περιαγωγης· Ωσπερ, &c. "But the moon is prevented from "falling by its motion, and the violence of its revolution; as bodies placed in a sling and "whirled round do not drop out. For every body will be carried according to its natural "motion, if not diverted by some other cause. The moon is not, therefore, carried in the "direction of (or by) its weight, because its circular motion opposes this tendency."

X.

A short Mineralogical Description of the Mountain of Gibraltar. By Major IMRIE*.

THE mountain of Gibraltar is situated in 36° 9′ north latitude, and in 5° 17′ east longitude from Greenwich. It is the promontory which, with that of Ceuta upon the opposite coast of Barbary, forms the entrance of the straits of Gibraltar from the Mediterranean; and Europa Point, which is the part of the mountain that advances most towards Africa, is generally regarded as the most southern promontory in Europe. The form of this mountain is oblong; its summit a sharp craggy ridge; its direction is nearly from north to south; and its greatest length in that direction falls very little short of three miles. Its breadth varies with the indentations of the shore, but it no where exceeds three quarters of a mile. The line of its ridge is undulated, and the two extremes are somewhat higher than its centre.

The summit of the Sugar-loaf, which is the point of its greatest elevation towards the south, is 1439 feet; the Rock Mortar, which is the highest point to the north, is 1350; and the Signal-house, which is nearly the central point between these two, is 1276 feet above the level of the sea. The western side of the mountain is a series of rugged slopes, interspersed with abrupt precipices. Its northern extremity is perfectly perpendicular, except towards the north-west, where what are called the Lines intervene, and a narrow passage of flat

* Transactions of the Royal Society of Edinburgh, iv. 191.

ground that leads to the iſthmus, and is entirely covered with fortification. The eaſtern ſide of the mountain moſtly conſiſts of a range of precipices; but a bank of ſand riſing from the Mediterranean in a rapid acclivity covers a third of its perpendicular height. Its ſouthern extremity falls in a rapid ſlope from the ſummit of the Sugar-loaf into a rocky flat of conſiderable extent called Windmill-hill. This flat forms half an oval, and is bounded by a range of precipices, at the ſouthern baſe of which a ſecond rocky flat takes place ſimilar in form and extent to Windmill-hill; and alſo like it ſurrounded by a precipice, the ſouthern extremity of which is waſhed by the ſea, and forms Europa Point. Upon the weſtern ſide this peninſular mountain is bounded by the bay of Gibraltar, which is in length nearly eight miles and a half, and in breadth upwards of five miles. In this bay the tide frequently riſes four feet. Upon the north the mountain is attached to Spain by a low ſandy iſthmus, the greateſt elevation of which above the level of the ſea does not exceed ten feet, and its breadth at the baſe of the rock is not more than three quarters of a mile. This iſthmus ſeparates the Mediterranean on the eaſt from the bay of Gibraltar on the weſt.

This mountain is much more curious in its botanical than in its mineralogical productions. In reſpect to the firſt, it connects in ſome degree the Flora of Africa with that of Europe. In reſpect to the latter, it produces little variety; perhaps a few ſubſtances and phenomena that are rare, but none that are peculiar.

The principal maſs of the mountain rock conſiſts of a grey denſe (what is generally called primary) marble; the different beds of which are to be examined in a face of 1350 feet of perpendicular height, which it preſents to Spain in a conical form. Theſe beds or ſtrata are of various thickneſs, from 20 to upwards of 40 feet, dipping in a direction from eaſt to weſt nearly at an angle of 35 degrees. In ſome parts of the ſolid maſs of this rock I have found teſtaceous bodies entirely tranſmuted into the conſtituent matter of the rock, and their interior hollows filled up with calcareous ſpar; but theſe do not occur often in its compoſition, and its beds are not ſeparated by any intermediate ſtrata.

In all parts of the globe where this ſpecies of rock conſtitutes large diſtricts, it is found to be cavernous. The caves of Gibraltar are many, and ſome of them of great extent. That which moſt deſerves attention and examination is called St. Michael's Cave, which is ſituated upon the ſouthern part of the mountain, almoſt equally diſtant from the Signal-tower and the Sugar-loaf. Its entrance is 1000 feet above the level of the ſea: this entrance is formed by a rapid ſlope of earth which has fallen into it at various periods, and which leads to a ſpacious hall incruſted with ſpar, and apparently ſupported in the centre by a large maſſy ſtalactitical pillar. To this ſucceeds a long ſeries of caves of difficult acceſs. The paſſages from the one to the other of theſe are over precipices, which can only be paſſed by the aſſiſtance of ropes and ſcaling-ladders. I have myſelf paſſed over many of theſe to the depth of 300 feet from the upper cave; but at that depth the ſmoke of our torches became ſo diſagreeable that we were obliged to give up our purſuit, and leave caves ſtill under us unexamined. In theſe cavernous receſſes, the formation and proceſs of ſtalactites is to be traced from the flimſy quilt-like cone, ſuſpended from the roof, to the robuſt trunk of a pillar, three feet in diameter, which riſes from the floor, and ſeems intended by nature to ſupport the roof from which it originated.

The variety of form which this matter takes in its different ſituations and directions renders this ſubterraneous ſcenery ſtrikingly groteſque, and in ſome places beautifully picturesque.

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The ſtalactites of theſe caves when near the ſurface of the mountain are of a browniſh yellow colour; but as we deſcended towards the lower caves we found them begin to loſe their darkneſs of colour, which by degrees ſhaded off to a whitiſh yellow.

The only inhabitants of theſe caves are bats, ſome of which are of a large ſize. The ſoil in general upon the mountain of Gibraltar is but thinly ſown; and in many parts that thin covering has been waſhed off by the heavy autumnal rains, which have left the ſuperficies of the rock for a conſiderable extent bare and open to inſpection. In thoſe ſituations an obſerving eye may trace the effects of the ſlow but conſtant decompoſition of the rock, cauſed by its expoſure to the air, and the corroſion of ſea ſalts, which in the heavy gales of eaſterly winds are depoſited with the ſpray on every part of the mountain. Thoſe uncovered parts of the mountain rock alſo expoſe to the eye a phenomenon worthy of ſome attention, as it tends clearly to demonſtrate, that, however high the ſurface of this rock may now be elevated above the level of the ſea, it has once been the bed of agitated waters. This phenomenon is to be obſerved in many parts of the rock, and is conſtantly found in the beds of torrents. It conſiſts of pot-like holes of various ſizes hollowed out of the ſolid rock, and formed apparently by the attrition of gravel or pebbles ſet in motion by the rapidity of rivers or currents in the ſea. One of thoſe, which had been recently laid open, I examined with attention. I found it to be five feet deep and three feet in diameter; the edge of its mouth rounded off as if by art, and its ſides and bottom retaining a conſiderable degree of poliſh. From its mouth for three feet and a half down it was filled with a red argillaceous earth, thinly mixed with minute parts of tranſparent quartz cryſtals: the remaining foot and a half to the bottom contained an aggregate of water-worn ſtones, which were from the ſize of a gooſe's egg to that of a ſmall walnut, and conſiſted of red jaſpers, yellowiſh white flints, white quartz, and blueiſh white agates, firmly combined by a yellowiſh brown ſtalactitical calcareous ſpar. In this breccia I could not diſcover any fragment of the mountain rock, or any other calcareous matter, except the cement with which it was combined. This pot is 940 feet above the level of the ſea.

Upon the weſt ſide of the mountain towards its baſe ſome ſtrata occur, which are heterogenial to the mountain rock: the firſt or higheſt forms the ſegment of a circle; its convex ſide is towards the mountain, and its ſlopes alſo in that direction. This ſtratum conſiſts of a number of thin beds; the outward one, being the thinneſt, is in a ſtate of decompoſition, and is mouldering down into a blackiſh brown or ferruginous coloured earth. The beds inferior to this progreſſively increaſe in breadth to 17 inches, where the ſtratification reſts upon a rock of an argillaceous nature.

This laſt bed, which is 17 inches thick, conſiſts of quartz of a blackiſh blue colour, in the ſepta or cracks of which are found fine quartz cryſtals, colourleſs and perfectly tranſparent. Theſe cryſtals are compoſed of 18 planes, diſpoſed in hexangular columns, terminated at both extremities by hexangular pyramids. The largeſt of thoſe that I have ſeen does not exceed two-eighths of an inch in length: they in general adhere to the rock by the ſides of the column, but are detached without difficulty. Their great degree of tranſparency has obtained them the name of Gibraltar diamonds.

[*To be continued.*]

ACCOUNT OF NEW BOOKS.

Memoirs of the Literary and Philoſophical Society of Mancheſter, Vol. V. Part I. 8vo. 318 pages, with four plates. Price 6 ſhillings in boards. Cadell and Davies, London, 1798.

THIS volume contains, 1. Curſory Remarks, moral and political, on Party Prejudice. By Samuel Argent Bardſley, M. D.—2. Extraordinary Facts relating to the Viſion of Colours; with Obſervations. By Mr. John Dalton. The intelligent author of this valuable paper is himſelf the ſubject of many of his obſervations reſpecting the peculiarity of his perceptions of colours. This Memoir conſiſts of an account of his own viſion; an account of others whoſe viſion has been found ſimilar to his own; and obſervations reſpecting the probable cauſe of the peculiarities he deſcribes. The ſolar ſpectrum, formed by the priſmatic diſperſion of light, exhibits to him only two, or at moſt three, colours, which he ſhould call yellow and blue, or yellow, blue, and purple. This yellow comprehends the red, orange, yellow, and green of others; and his blue and purple coincide with theirs. He enters minutely into the requiſite comparative obſervations on the ſeveral colours reſpectively as they affect his organs of ſight. Pink appears, by day-light, to be ſky-blue a little faded; by candle-light it aſſumes an orange or yellowiſh appearance, which forms a ſtrong contraſt to blue. Crimſon appears a muddy blue by day; and crimſon woollen yarn is much the ſame as dark blue. Red and ſcarlet have a more vivid and flaming appearance by candle-light than by day-light. There is not much difference in colour between a ſtick of red ſealing-wax and graſs by day. Dark green woollen cloth ſeems a muddy red, much darker than graſs, and of a very different colour. The colour of a florid complexion is duſky blue. Coats, gowns, &c. appear to Mr. Dalton, and the other individuals he mentions, frequently to be badly matched with linings, when others ſay they are not. On the other hand, they ſhould match crimſons with claret or mud; pinks with light blues; browns with reds; and drabs with greens. In all points where they differ from other perſons, the difference is much leſs by candle-light than by day-light. Mr. Dalton, by various obſervations, ſhews, almoſt beyond a doubt, that this affection of the eye is cauſed by ſome of the humours, probably the vitreous, being coloured by ſome modification of blue.—3. An Enquiry into the Name of the Founder of Huln Abbey, Northumberland, the firſt in England of the Order of Carmelites; with Remarks on Dr. Ferriar's Account of the Monument in the Church of that Monaſtery. By Robert Uvedale, B. A. of Trinity College, Cambridge.—4. On the Variety of Voices. By Mr. John Gough. It is a fact which continually preſents itſelf to our obſervation, that ſounds differ from each other in other reſpects as well as in muſical tone and their intenſity. The oboe, the violin, and the flute, however perfectly they may be made to agree in theſe reſpects, are ever found to differ in ſomething which may be called the character of the tone itſelf. The author of this paper conceives that the variety of human voices is of this laſt kind; and, in his reaſoning on the philoſophy of ſound, he conſiders every tone as compounded. So that the ſame cotemporaneous ſounds which are heard in the bell, though practically conſidered as if it emitted the fundamental note only, being conceived to exiſt in all other tones, theſe will be found to differ according to the number, the nature or relation, and the intenſity of their component parts. That circum-

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ſtances of this kind may modify the aggregate of ſonorous undulation, is ſcarcely to be doubted; but Mr. Gough ſeems to have overlooked that modification, which may be proved to be the diſtinguiſhing circumſtance between tone and tone when they are in uniſon. If a toothed wheel be made to revolve very ſwiftly with its teeth againſt a ſpring, which ſhall ſtrike each tooth in ſucceſſion, a muſical tone will be produced, which will be more acute the ſwifter the rotation. But this tone will differ accordingly as the material of the wheel is itſelf diſpoſed to give a tone of greater acuteneſs or gravity by each ſingle or individual ſtroke. If the ſame note be afforded by a braſs and by a wooden wheel revolving on one axis, the character of each note will differ in the manner here ſtated. Or more ſimply, if a cord of thirty or forty feet in length, or ſhorter if more convenient, be ſtretched, and in this ſtate ſtruck with the edge of the hand, the vibrations will be ſlow enough to be counted, and will evidently appear to be of two kinds. When the ſtring is ſtruck near the end or bridge, a wave or undulation will be propagated to the other bridge, and immediately return; which proceſs will continue as long as the elaſticity of the ſtring can maintain the motion. The waves of this undulation will be larger or ſmaller, the more remote or the nearer the place of percuſſion is to the bridge; but the times of tranſmiſſion of the waves backwards and forwards along the ſtring will be invariable. The tone of ſuch a ſtring, ſuppoſing its dimenſions and tenſion to be ſuch as would produce ſound, may therefore be conſidered as if the tone produced by the ſyſtem of waves were interrupted as often as the re-action of each bridge cauſes the retrograde motion of the ſyſtem; that is to ſay, it is afforded by a proceſs ſimilar to that of the experiment with the wheels, where the peculiar ſound of a blow upon each tooth was inceſſantly repeated and ſuſpended at regular intervals. Now, the muſical acuteneſs or gravity of the tone depends altogether on the time of theſe intervals, and not at all on the nature of the ſound which is thus repeated; but the character of the tone is governed by this laſt circumſtance. And accordingly it is found by thoſe who make harpſichords, and other ſimilar inſtruments, that the character of the tone of the ſame ſtring is wonderfully changed, accordingly as the jacks or hammers are made to operate nearer to, or further from, the bridge.—5. On the Benefits and Duties reſulting from the Inſtitution of Societies for the Advancement of Literature and Philoſophy. By the Reverend Thomas Giſborne, M. A.—6. On an Univerſal Character. By James Anderſon, LL.D. F.R.S. &c. By this letter, which bears date February 20, 1795, it appears that the Doctor had then made conſiderable progreſs in the inveſtigation of the means of writing language by characters regularly conſtructed on thoſe principles of mental operation which conſtitute univerſal grammar, and do not require the intervention of ſound. To thoſe who have conſidered this ſubject, particularly with reſpect to the practice of the Chineſe, whoſe written words are known to repreſent, for the moſt part, things inſtead of words, and conſequently are intelligible to nations who do not ſpeak the ſame language, the practicability of this ſcheme will be a matter of no doubt. But whether the difficulties be ſuch as to operate more ſtrongly againſt its introduction in Europe than the immediate motives of convenience in favour of its reception, is a queſtion which can only be decided by minute and continued attention to the whole ſubject. That theſe difficulties are comparatively ſmall, and would yield to the induſtry of active and intelligent men, in the firſt inſtance, is ſcarcely to be doubted, provided the exertions of the firſt promoters of this ſcheme were carried ſo far as to afford a grammar, a dictionary, and one or two well written intro-

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ductory books on morals, political economy, or the more confined sciences of mechanics, chemistry, or astronomy. The facility and accuracy of enunciation, the speed and brevity of description, and the extreme rapidity of perusal, would soon realize those expectations upon which our author and a few men of reflection now meditate as the objects of enjoyment destined for remote future ages. The Doctor has not entered into the particulars of his own scheme; and the present occasion does not admit of a detail of the multiplied means and advantages which present themselves in the contemplation of this interesting subject. From the date of his letter, it is to be feared that other avocations may have suspended this pursuit, as well on his own part as that of his able friend whom he mentions as being actively engaged in it.—7. The Inverse Method of Central Forces. Communicated by Dr. Holme.—8. Observations on Iron and Steel. By Joseph Collier. After a concise account of the methods of smelting and refining iron, a description is given of the manufacture of steel by cementation, with a good drawing exhibiting the plan and section of a furnace for performing this operation. I do not recollect meeting with a drawing of this kind in any English author. Mr. Collier's paper exhibits a summary of the most essential facts related by Reaumur, Duhamel, Vandermonde, Monge, and Berthollet, and others*.—9. Remarks on Dr. Priestley's Experiments and Observations relating to the Analysis of Atmospherical Air, and his Considerations on the Doctrine of Phlogiston and the Decomposition of Water. By Theophilus Lewis Rupp. The modern chemistry which rejects phlogiston is ably defended in this paper, of which, from the close method of quotation and remark according to which it is written, it is impossible to give any fair account in this place.—10. An Account of three different Kinds of Timber Trees, which are likely to prove a great Acquisition to this Kingdom, both in point of Profit, and as Trees for Ornament and Shade. By Charles White, Esq. F.R.S. The trees are the black American birch with broad leaves, the Athenian poplar, and the iron oak with prickly cups. For the minute particulars of description, as well as the perspicuous detail of advantages, recourse must be had to the paper itself.—11. An Analysis of the Waters of two Mineral Springs at Lemington Priors, near Warwick, including Experiments tending to elucidate the Origin of the Muriatic Acid. By William Lambe, M. A. late Fellow of St. John's College, Cambridge.—12. Some Account of the Persian Cotton Tree. By Matthew Guthrie, M.D. F.R.S. &c. — 13. Experiments and Observations on the Preparation and some remarkable Properties of the Oxygenated Muriate of Potash. By Thomas Hoyle, jun.—14. Experiments and Observations on Fermentation and the Distillation of Ardent Spirit. By Joseph Collier. On the chemical papers last mentioned, as well as the subsequent paper on bleaching, I make no remarks, not only for the sake of brevity, but because I hope on a future occasion to give a more ample account of their contents.—15. Hints on the Establishment of an Universal Written Character. By William Brown, M.D. Notwithstanding the great value of this paper, and the ability of its author, who has occasionally turned his thoughts during three years to Dr. Anderson's project, I cannot help wishing that he may not be the friend to whom that author refers. His hints contain an outline of the process by which an universal written character is to be established; but he seems rather disposed to consider the pro-

* The author mentions certain omissions in my Chemical Dictionary, article Iron, which he would have found under the word Steel, if he had turned to that article.

ject as being very laborious, and in its practical application difficult to be established. Under such impressions, much is not to be expected in the progress. It seems nevertheless that mankind have rejected universal characters when offered to them, not because they were averse from the means of facility and perspicuity, but because those means were not in reality displayed. The Arabic numerals, the notation of music, and the symbols of quantity and operation used by algebraists, are universal characters confined to no language, and have been universally received because they are simple and appropriate; but the characters of chemists, as well as the few attempts at the more ample designation of things in the same manner, have not been received, because their authors have stopped short in their use and adaptation. If Bishop Wilkins, and the great men of the last century who favoured his universal character, had studied and used it to such an extent as to have written treatises according to that notation; or if the great Bergman had attempted a reform in the characters of chemistry, and adopted such as could be readily formed, easily distinguished, and compounded, with all the advantages of position, of which he was so well aware; if he had completed a set of tables exhibiting the whole of chemical science, and expressing things, quantities, actions, temperatures, and other habitudes, in a few pages, which those who attentively meditate on his compound tables of elective attraction may without difficulty conceive to be possible; the lovers of that science would have learned to read his work, and to write others according to the same method; and that for reasons of the same kind as have induced arithmeticians, musicians, and algebraists, to become masters of the universal characters proper to their respective sciences.—16. On the Process of Bleaching with the Oxygenated Muriatic Acid, and a Description of a new Apparatus for Bleaching Cloths with that Acid dissolved in Water, without the Addition of Alkali. By Theophilus Lewis Rupp.—17. Account of a remarkable Change of Colour in a Negro. By Miers Fisher. This man's father was the son of a native African and an Indian of Philadelphia. His mother was the daughter of an African man and an Irish woman. By a certificate here exhibited, it appears, that until the month of February 1792, being then at least 30 years old, he was of as dark a complexion as any African; at which period his skin began to change white, like that of an European, commencing at his fingers' ends, and proceeding, chiefly in the summer or warm weather, over the rest of his body. The greater part of the surface was thus changed at the time of the account being written, which was the 22d of November 1796. The change was not uniform over the whole surface, but gradually progressive along the skin; the black and white parts being separated by an irregular line. No facts are mentioned in explanation of this remarkable change.

Transactions of the Royal Society of Edinburgh, Vol. IV. Quarto. 470 pages, with 12 plates. Printed for Cadell and Davies in London; and Dixon and Balfour, Edinburgh, 1798.

This work is divided into three parts.

Part I. contains the History of the Society, or Reports concerning Memoirs presented, and Communications made, with a List of the Officers and Members, Donations, and other similar Particulars. It also contains Biographical Accounts of Lord Abercromby; of William Tytler, Esq. of Woodhouselee; of Mr. William Hamilton, late Professor of Anatomy and Botany in the University of Glasgow; and of John Roebuck, M. D.

Part II.

Part II. contains, I. Papers of the Phyſical Claſs.—1. Account of a Mineral from Strontian, and of a peculiar Species of Earth which it contains. By Thomas Charles Hope, Profeſſor* of Medicine in the Univerſity of Glaſgow.—2. Obſervations on the Natural Hiſtory of Guiana. By William Lochead, Eſq.—3. On the Principles of the Antecedental Calculus. By James Glenie, Eſq.—4. Obſervations on the Trigonometrical Tables of the Brahmins. By John Playfair, Profeſſor of Mathematics in the Univerſity of Edinburgh.—5. Some Geometrical Poriſms, with Examples of their Application to the Solution of Problems. By Mr. William Wallace, Aſſiſtant Teacher of the Mathematics in the Academy of Perth.—5†. On the Latitude and Longitude of Aberdeen. By Andrew Mackay, LL.D. & F.R.S. Edin.—6. An Account of certain Motions which ſmall lighted Wicks acquire when ſwimming on a Baſon of Oil. By Patrick Wilſon, F.R.S. Edin. and Profeſſor of Practical Aſtronomy in the Univerſity of Glaſgow. (See Philoſ. Journal, II. 167.)—7. Account of a ſingular Halo of the Moon. By William Hall, Eſq. of Whitehall, F.R.S. Edin.—8. A New Series for the Rectification of the Ellipſis, with Obſervations on the Evolution of a certain Algebraic Formula. By James Ivory, A.M.—9. Mineralogical Deſcription of the Mountain of Gibraltar. By Major Imrie.—10. Deſcription of a Thermometer, which marks the greateſt Degree of Heat and Cold from one Time of Obſervation to another. By Alexander Keith, Eſq. F.R.S. & F.A.S. Edin.—11. Deſcription of a Barometer which marks the Riſe and Fall of the Mercury from two different Times of Obſervation. By Alexander Keith, Eſq. F.R.S. & F.A.S. Edin.—12. Meteorological Abſtract for the Years 1794, 1795, 1796.———II. Papers of the Literary Claſs.—1. On the Origin and Principles of Gothic Architecture. By Sir James Hall, Bart. F.R.S. & A.S.S. Edin.—2. M. Chevalier's Tableaude la Plaine de Troye illuſtrated and confirmed from the Obſervations of ſubſequent Travellers and others. By Andrew Dalzel, M.A. F.R.S. Edin. Profeſſor of Greek in the Univerſity of Edinburgh.

* Now Joint Profeſſor of Chemiſtry in the Univerſity of Edinburgh.

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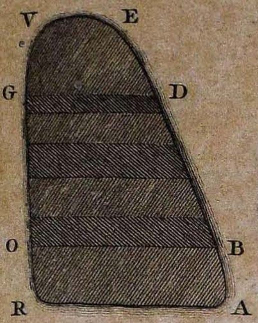

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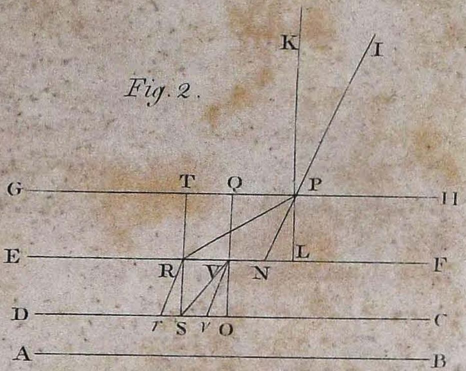

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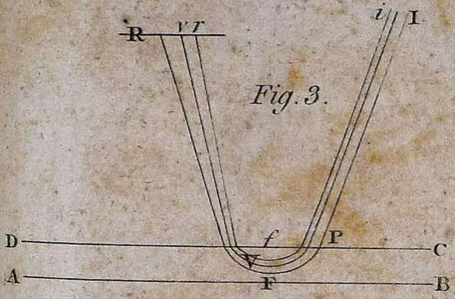

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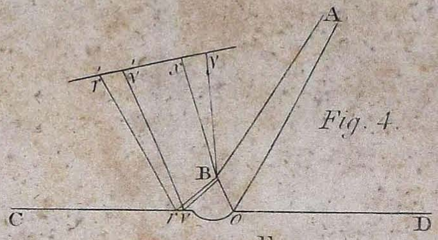

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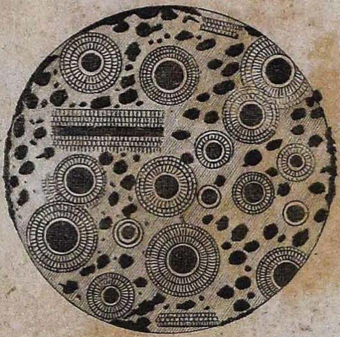

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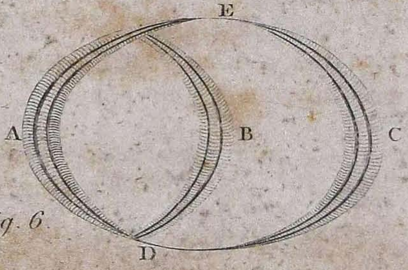

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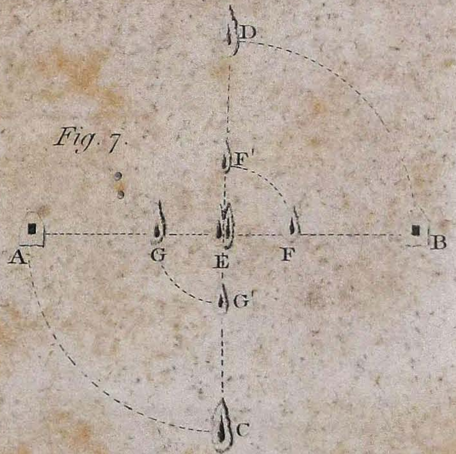

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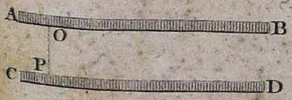
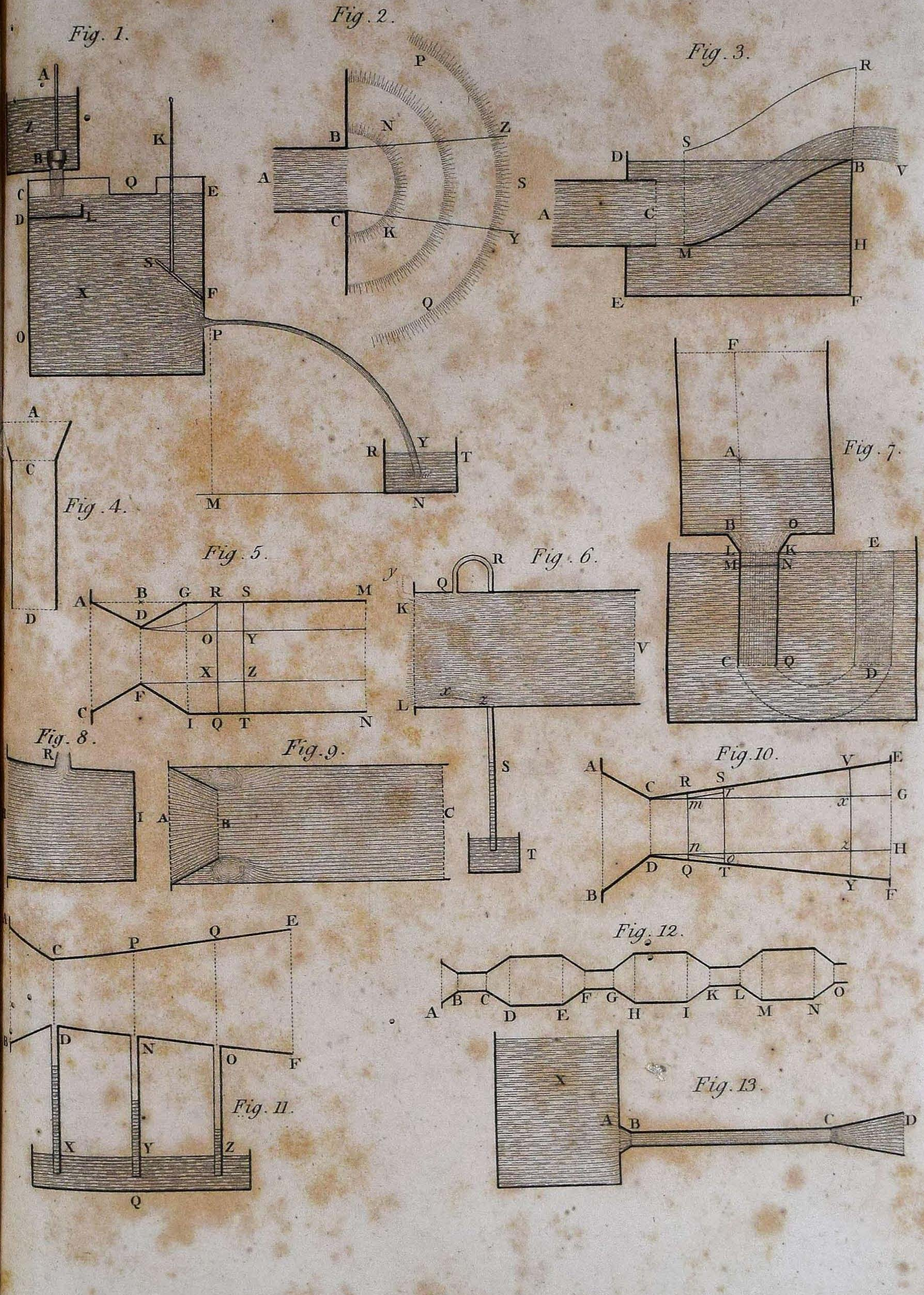


Barlow sculp.



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